DOCUMENT RESUME

ED 386 572 CE 069 815

AUTHOR	Benus, Jacob M.; And Others
TITLE	Self-Employment Programs: A New Reemployment Strategy, Final Report on the UI Self-Employment Demonstration. Unemployment Insurance Occasional Paper 95-4.
INSTITUTION	Abt Associates, Inc., Bethesda, MD.
SPONS AGENCY	Employment and Training Administration (DOL), Washington, D.C.
PUB DATE	95
CONTRACT	99-8-0803-98-047-01
NOTE	257p.; For related reports, see CE 069 813-814.
PUB TYPE	Reports - Research/Technical (143)
EDRS PRICE	MF01/PC11 Plus Postage.
DESCRIPTORS	Adult Education; Cost Effectiveness; *Demonstration Programs; *Employment Programs; Entrepreneurship; Federal Legislation; Job Development; Job Training; Program Development; Program Effectiveness; Program Implementation; Salaries; *Self Employment; Small Businesses; State Programs; *Unemployment; Unemployment Insurance; Wages
IDENTIFIERS	Massachusetts; *North American Free Trade Agreement; Washington

ABSTRACT

The main parameters of state self-employment assistance (SEA) programs for unemployed workers were tested in two experimental demonstrations authorized by the North American Free Trade Agreement Implementation Act. The Washington State and Massachusetts Unemployment Insurance (UI) Self-Employment Demonstrations evaluated the ability of the employment security and economic development systems to work together and help UI recipients create their own jobs by starting businesses. The classical experimental design randomly assigned applicants to either a treatment or a control group. Design differences between the two demonstrations were a requirement in the Massachusetts authorizing legislation that eliminated those with a low predicted probability of exhausting UI benefits and eligibility of Washington participants to receive a lump-sum payment of remaining benefits. The demonstrations were initiated in six Washington and seven Massachusetts sites. Relatively few new UI claimants in both states chose to pursue self-employment when the opportunity arose. Findings from a follow-up survey approximately 31 months after assignment in Massachusetts and approximately 33 months in Washington, indicated that both demonstrations increased the likelihood of self-employment, increased the total time in employment, and reduced the length of unemployment. Both programs were cost effective from the nonparticipant and government perspectives. (Appendixes include definitions of independent variables, significant coefficients, and effects on subgroups.) (YLB)

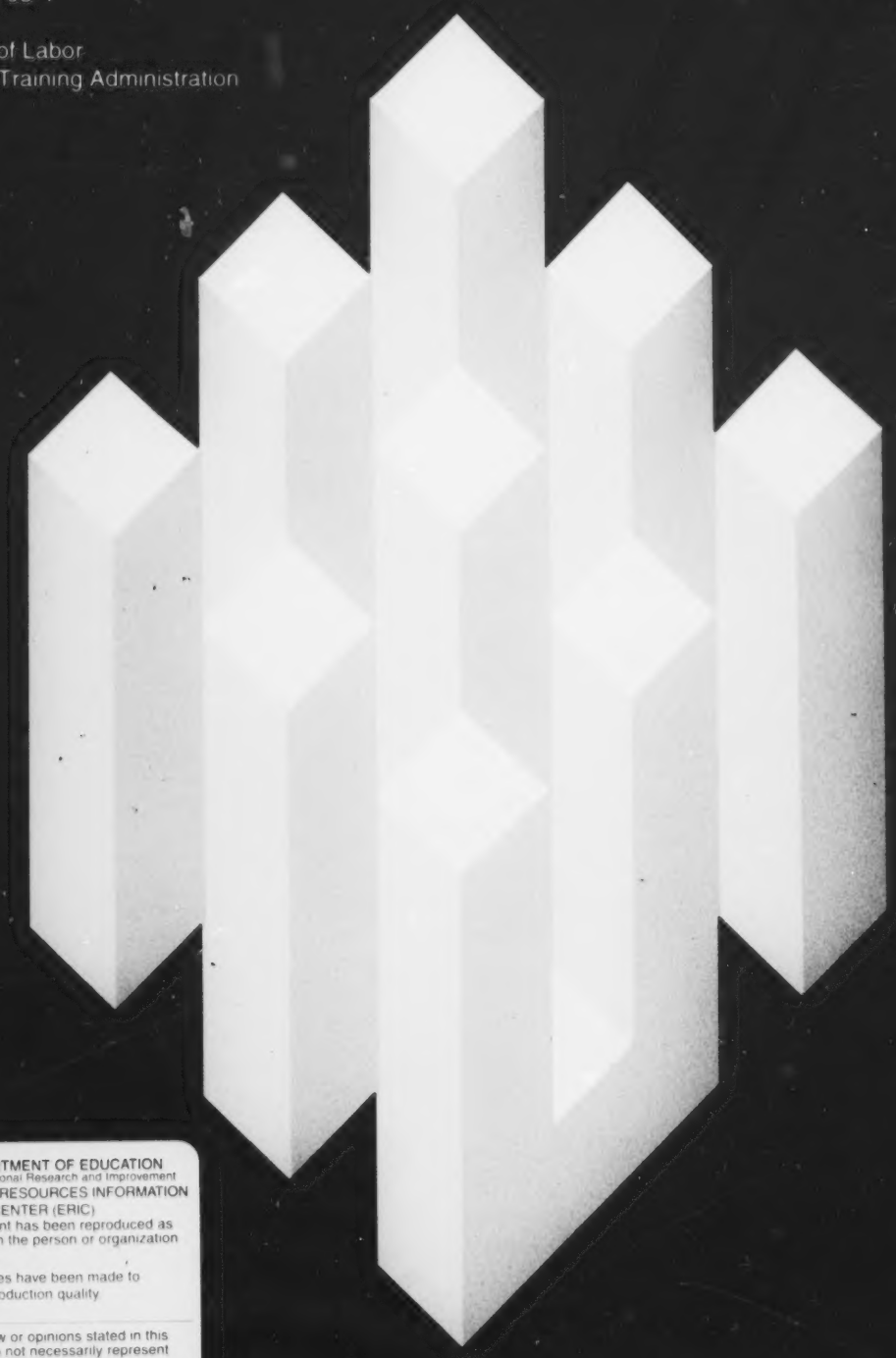
ED 386 572

CE

Self-Employment Programs: A New Reemployment Strategy, Final Report on the UI Self-Employment Demonstration

Unemployment Insurance
Occasional Paper 95-4

U.S. Department of Labor
Employment and Training Administration



CE069 815

U.S. DEPARTMENT OF EDUCATION
Office of Educational Research and Improvement
EDUCATIONAL RESOURCES INFORMATION CENTER (ERIC)

- ☐ This document has been reproduced as received from the person or organization originating it.
- ☐ Minor changes have been made to improve reproduction quality.
- • Points of view or opinions stated in this document do not necessarily represent official OERI position or policy.

BEST COPY AVAILABLE

Material contained in this publication
is in the public domain and may be
reproduced, fully or partially, without
permission of the Federal Government.
Source credit is requested but not
required. Permission is required
only to reproduce any copyrighted
material contained herein.

Self-Employment Programs: A New Reemployment Strategy, Final Report on the UI Self-Employment Demonstration

Unemployment Insurance
Occasional Paper 95-4

U.S. Department of Labor
Robert B. Reich, Secretary

Employment and Training Administration
Doug Ross, Assistant Secretary

Unemployment Insurance Service
Mary Ann Wyrsch, Director

1995

This report was prepared for the U.S. Department of Labor, Employment and Training Administration, Unemployment Insurance Service under contract number 99-8-0803-98-047-01. The authors of this report are Jacob M. Benus, Terry R. Johnson, Michelle Wood, Neelima Grover, and Theodore Shen. Since contractors conducting research and evaluation projects under government sponsorship are encouraged to express their own judgement freely, this report does not necessarily represent the official opinion or policy of the U.S. Department of Labor.

The UIOP Series presents research findings and analyses dealing with unemployment insurance issues. Papers are prepared by research contractors, staff members of the unemployment insurance system, or individual researchers. Manuscripts and comments from interested individuals are welcome. All Correspondence should be sent to:

UI Occasional Papers,
Unemployment Insurance Service,
Frances Perkins Building, Room S-4519,
200 Constitution Ave., N.W.
Washington, DC 20210
Attention: Rosalind Thomas

SELF-EMPLOYMENT PROGRAMS
A NEW REEMPLOYMENT STRATEGY

Final Impact Analysis of the Washington and Massachussets Self-Employment Demonstrations

December 1994

Authors:

Jacob M. Benus (Abt Associates)
Terry R. Johnson (Battelle)
Michelle Wood (Abt Associates)
Neelima Grover (Abt Associates)
Theodore Shen (Abt Associates)

Submitted to:

USDOL/ETA/UIS
200 Constitution Avenue, NW
Room S-4519
Washington, DC 20210
Attention: Jon Messenger

Submitted By:

Abt Associates Inc.
Hampden Square, 6th Floor
4800 Montgomery Lane
Bethesda, Maryland 20814
(301) 913-0500

This report was prepared for the U.S. Department of Labor, Employment and Training Administration, Unemployment Insurance Service under Contract No. 99-8-0803-98-047-01. Since contractors conducting research and development projects are encouraged to state their findings and express their judgments freely, this report does not necessarily reflect the official opinion or policy of the U.S. Department of Labor. The contractors are solely responsible for the content of this report.

ACKNOWLEDGMENTS

The design, implementation, and analysis of the Unemployment Insurance (UI) Self-Employment Demonstration, which included both the Washington Self-Employment and Enterprise Development (SEED) Project and the Massachusetts Enterprise Project, required an extensive collaborative effort among the U.S. Department of Labor (USDOL), the Washington State Employment Security Department, the Washington State Department of Trade and Economic Development, the Massachusetts Department of Employment and Training, the Massachusetts Office of Business Development, Abt Associates Inc., and Battelle Memorial Institute, and a number of local organizations that provided business assistance services to participants in the two projects. Many individuals from these and other organizations have made valuable contributions to the success of the Washington and Massachusetts demonstrations.

The authors wish to particularly acknowledge a number of USDOL staff who contributed greatly to the success of the project. We particularly appreciate the support of Jon Messenger, the USDOL Project Officer. Jon, together with Dr. Stephen Wandner, provided guidance and valuable input throughout the research design, implementation, and analysis stages of the project. We also appreciate the assistance of Wayne Gordon and John Chang who contributed in a number of areas.

The authors are indebted to staff of the SEED unit of the Washington State Employment Security Department. The day-to-day operations of the project were managed by Judy Johnson, under the direction of Kathy Countryman. Judy was involved in virtually all aspects of the demonstration program, including the training of demonstration staff, reviewing applications, implementing random assignment, monitoring program operations and dealing with various automated data systems. Others in the state that deserve our recognition include Davis Garabato, who was actively involved in the monitoring of demonstration activities, Eric Jordan who was instrumental in ensuring the quality of the administrative data used for analysis, and Gary Bodeutsch, who provided information on state and local area characteristics.

We are also grateful for the assistance of Earl True and Claire Phillips from the Washington Department of Trade and Economic Development. Earl and Claire hired, trained and monitored the business development specialists and helped ensure the provision of high-quality business services data.

In Massachusetts, we would like to acknowledge the efforts of Bonnie Dallinger, the Enterprise Project Manager. The day-to-day operations of the Enterprise Project were managed by Bonnie, under the supervision of the Department of Employment and Training Commissioner, Nils Nordberg. Bonnie was involved throughout the design and implementation of the demonstration, including the selection of business training providers, training local office staff, establishing the application review committee, and monitoring all demonstration operations. Other members of Bonnie's staff who made valuable contributions were Jean Kluver, Ilene Ladd, Victor Ramirez, Julie Fuller, Leslie Friedman, Deb Mapes, and Maureen Fritz.

The success of the demonstration owes much to Jan Leonard and David Sommers of Battelle. Jan did the bulk of the site monitoring and helped author the interim report. David did an excellent job in performing data analysis tasks. Within Abt, we would like to thank Maureen Meagher, who directed the survey data collection, Robert Teitel, who supervised the survey data file programming, Neelima Grover and Ted Shen, who constructed many of the analysis variables and performed many complex analysis tasks, David Bell and Elizabeth Moon, who implemented numerous analysis tasks, and Wendy Davis, who oversaw the production of this report. We would also like to thank Larry Orr, a co-principal investigator of the SEED Project, who provided invaluable guidance throughout the project and made helpful comments which improved this report.

ABSTRACT

The North American Free Trade Agreement (NAFTA) Implementation Act (P.L. 103-182) authorizes states to establish self-employment assistance (SEA) programs for unemployed workers. As of the end of 1994, five states (California, Connecticut, Maine, New York and Rhode Island) have enacted enabling legislation to implement SEA programs for the unemployed.

The main parameters of SEA programs were tested in two experimental demonstrations sponsored by the U.S. Department of Labor (DOL). The Washington State and Massachusetts Unemployment Insurance Self-Employment Demonstrations evaluated the ability of the U.S. employment security and economic development systems to work together and help Unemployment Insurance (UI) recipients create their own jobs by starting businesses. Preliminary results from these two demonstrations indicated that SEA is a viable reemployment option for some portion of the unemployed. Moreover, preliminary results suggested that the Massachusetts model, was likely to be a cost-effective approach for providing SEA to UI claimants. These early results were cited in the decision to authorize SEA for a five-year period.

In this report, we present the final impact estimates of the Washington and Massachusetts UI Self-Employment Demonstrations. These final results largely reinforce the earlier preliminary findings and underscore the conclusion that SEA is a viable policy tool to promote the rapid reemployment of unemployed workers. The cumulative evidence from the preliminary and final evaluations suggests that SEA should be permanently incorporated into the U.S. employment security and economic development system.

TABLE OF CONTENTS

EXECUTIVE SUMMARY i

CHAPTER 1 INTRODUCTION 1

Background of the UI Self-Employment Demonstrations 2
Washington Self-Employment and Enterprise Development (SEED) Demonstration 3
The Massachusetts UI Self-Employment Demonstration (The Enterprise Project) 3
Organization of the Report 4

CHAPTER 2 EXPERIMENTAL AND OPERATIONAL DESIGNS 7

Targeting Demonstration Participants 7
Recruiting Participants 9
Application and Random Assignment 10
Business Start-up Services and Financial Assistance 11
Participation Requirements 15

CHAPTER 3 WASHINGTON DEMONSTRATION IMPLEMENTATION 16

Demonstration Intake 16
Business Support Services 23
Financial Assistance 27

CHAPTER 4 MASSACHUSETTS DEMONSTRATION IMPLEMENTATION 30

Demonstration Intake 30
Participant Characteristics 38
Changes in Program Implementation 38
Participation in Enterprise Project Activities 44
Timing of Enterprise Project Activities 46
Changes in UI Regulations 46

CHAPTER 5 COMPARISON OF DEMONSTRATION IMPLEMENTATION 53

Timing of the Intake Periods 53
Attendance at Orientation 55
Application 61
Random Assignment 61
Timing of Intake Activities 61
Program Experiences 64
Participant Satisfaction with Business Services 65

CHAPTER 6 DATA SOURCES 68

Participant Tracking System 68
Administrative Records 69
Survey Data 73

CHAPTER 7 WASHINGTON DEMONSTRATION IMPACTS 81

Estimation of Demonstration Impacts 82
Impact Results Using Administrative Data 105
Impacts of SEED for Various Claimant Subgroups 124
Summary 125

CHAPTER 8 MASSACHUSETTS DEMONSTRATION IMPACTS 129

Impacts Based on Cohorts 1 and 2 130
Impacts based on Cohort 3 146
Impacts for Various Claimant Subgroups 151
Summary 152

CHAPTER 9 COMPARISON OF DEMONSTRATION IMPACTS 155

Identification of a Washington Comparison Sample 155
Impacts on Self-Employment 157
Impacts on Wage and Salary Employment 163
Impacts on Total Employment and Earnings 168
Impacts on Job Creation 172
Impacts on UI Outcomes 173

CHAPTER 10 BENEFITS AND COSTS OF THE SELF-EMPLOYMENT DEMONSTRATIONS 176

Benefit-Cost Analysis 177
Measurement Of Outcomes 182
Net Benefits and Costs 191
Sensitivity of the Net Benefit Estimates 197

APPENDIX A -- DEFINITION OF INDEPENDENT VARIABLES

APPENDIX B -- SIGNIFICANT COEFFICIENTS

APPENDIX C -- EFFECTS ON SUBGROUPS

LIST OF EXHIBITS

Exhibit 2.1 Enterprise Project and Seed Project Intake and Assignment Process 8

Exhibit 2.2 Business Startup Services 12

Exhibit 3.1 Washington SEED Demonstration Sites 18

Exhibit 4.1 Probability of UI Benefit Exhaustion by Cohort 33

Exhibit 4.2 Massachusetts Enterprise Project Demonstration Sites During the Three Implementation Phases 42

Exhibit 4.3 Weeks of UI Benefits by Treatment and Control Cohorts 1, 2, and 3 49

Exhibit 4.4 Weeks of UI Benefits by Treatment and Control Cohorts 1 and 2, and Cohort 3 50

Exhibit 5.1 Demonstration Enrollment Over Time 54

Exhibit 5.2 Participation in Various Project Activities: Massachusetts and Washington 56

Exhibit 5.3 Washington Invitation Letter 58

Exhibit 5.4 Massachusetts Invitation Letter 59

Exhibit 6.1 Timing of Wave I and Wave II Surveys in Washington and Massachusetts 75

Exhibit 10.1 Social Benefits and Costs of the Self Employment Demonstrations 118

Exhibit 10.2 Government Benefits and Costs of the Self Employment Demonstrations 180

EXECUTIVE SUMMARY

The North American Free Trade Agreement (NAFTA) Implementation Act (P.L. 103-182) authorizes states to establish self-employment assistance (SEA) programs for unemployed workers. As of the end of 1994, five states (California, Connecticut, Maine, New York and Rhode Island) have enacted enabling legislation to implement SEA programs for the unemployed.

The main parameters of SEA programs were tested in two experimental demonstrations sponsored by the U.S. Department of Labor (DOL). The Washington State and Massachusetts Unemployment Insurance Self-Employment Demonstrations evaluated the ability of the U.S. employment security and economic development systems to work together and help Unemployment Insurance (UI) recipients create their own jobs by starting businesses. Preliminary results from these two demonstrations indicated that SEA is a viable reemployment option for some portion of the unemployed. Moreover, preliminary results suggested that the Massachusetts model, was likely to be a cost-effective approach for providing SEA to UI claimants. These early results were cited in the decision to authorize SEA for a five-year period.

In this report, we present the final impact estimates of the Washington and Massachusetts UI Self-Employment Demonstrations. These final results largely reinforce the earlier preliminary findings and underscore the conclusion that SEA is a viable policy tool to promote the rapid reemployment of unemployed workers. The cumulative evidence from the preliminary and final evaluations suggests that SEA should be permanently incorporated into the U.S. employment security and economic development system.

THE UI SELF-EMPLOYMENT DEMONSTRATIONS

The programs discussed in this report -- the Washington State Self-Employment and Enterprise Development (SEED) Project and the Massachusetts Enterprise Project -- are the first two federally-sponsored self-employment demonstration programs designed to assist unemployed workers in the United States. Collectively, they are known as the Unemployment Insurance (UI) Self-Employment Demonstrations. Abt Associates Inc. and Battelle Memorial Institute designed and evaluated these experimental demonstrations.

Demonstration Design

A classical experimental design was developed to rigorously evaluate program effectiveness. Applicants to the demonstration were randomly assigned either to a treatment group, which was eligible to receive all program services, or to a control group, which was not eligible to receive program services, but remained eligible for regular UI benefits.

To promote early intervention, both the Massachusetts and the Washington projects targeted new UI claimants. Both excluded persons filing interstate claims, claimants who were employer-attached (i.e., on standby to return to their former employer), and claimants under 18 years of age.

While there were some minor differences in program design between the two demonstrations, a key design difference resulted from the authorizing legislation for the Massachusetts demonstration (Section 9152 of the Omnibus Budget Reconciliation Act of 1987). The legislation required that the demonstration program focus on UI claimants who were likely to exhaust their UI benefits. To implement this legislative requirement, sample selection was based on a statistical model that predicted each new claimant's likelihood of UI benefit exhaustion. Those new claims with a low predicted probability of exhausting UI benefits were eliminated from the Massachusetts target group.

A second major design difference was the financial assistance component in each of the demonstrations. In Massachusetts, participants were eligible to receive periodic self-employment assistance payments. In Washington, participants were eligible to receive similar periodic

payments. In addition, however, upon meeting certain milestones in the development of their businesses, Washington participants were eligible to receive their remaining available UI benefits in one lump-sum payment.

Otherwise, the Washington and Massachusetts demonstration designs were very similar. Specifically, invited claimants were required to attend an orientation meeting and to submit an application to the program. Applicants who met all the requirements were randomly assigned to either the treatment or the control group. Treatment group members were then offered business start-up services and financial assistance.

Business Start-up Services and Financial Assistance

The first step for Massachusetts participants was to attend an Enterprise Seminar, a one-day training session conducted by one or more business experts. Within two weeks of the Enterprise Seminar, participants were required to attend an individual counseling session with their business counselor. Massachusetts participants were also required to attend six workshop sessions on a variety of business topics.

The financial assistance component in Massachusetts included payment of regular bi-weekly UI benefits, with an exemption from the regular UI work search requirements while in the demonstration. The demonstration design set the duration of the UI work search waiver at 24 weeks. Thus, in Massachusetts, treatment group members could collect self-employment allowances through the 24th consecutive week of their UI claim. Since Massachusetts claimants are eligible for up to 30 weeks of UI benefits, after 24 weeks they were required to choose between continuing with their self-employment activities full-time or returning to UI for the remaining six weeks of UI eligibility and meeting the work search requirements.

The business start-up services provided in the Washington demonstration differed in several respects from the Massachusetts demonstration. Within two weeks after random assignment, treatment group members were scheduled to attend a set of four business training sessions. The four Washington training sessions were presented during a one-week period; in contrast, the seven Massachusetts sessions were presented over a 12-week period.

The financial assistance component of the Washington demonstration included both regular UI payments and eligibility for a lump-sum payment. That is, Washington treatment group members received regular bi-weekly UI payments while engaged in business start-up activities; in addition, they were eligible to receive a lump-sum payment equal to their remaining UI entitlement when they achieved five program milestones:

- Completion of the training sessions;
- Development of an acceptable business plan;
- Establishment of a business bank account;
- Satisfying all licensing requirements; and
- Obtaining adequate financing.

There was no lump-sum payment available to Massachusetts participants.

Demonstration Implementation

The Washington SEED Demonstration was initiated on a pilot basis in one site in September 1989, and was then implemented in five additional sites in February 1990. Demonstration intake activities continued through September 1990, with business support services available to demonstration participants through March 1991. In the six SEED demonstration sites, a total of 755 new claimants were offered demonstration services (i.e., assigned to the treatment group); a total of 752 new claimants who applied to SEED were assigned to the control group.

The Massachusetts Enterprise Project began operations in May 1990. Enrollment into the Enterprise Project took place in three distinct phases: the first enrollment phase took place in 1990 (May - September), the second in 1991 (April - October), and the third in 1992-93 (March 92 - April 93). Over all three phases of operation, a total of 614 new UI claimants were enrolled in the Enterprise Project in the seven sites; an additional 608 new UI claimants were assigned to the control group.

In both Washington and Massachusetts, only a relatively small fraction of targeted UI claimants met the initial demonstration requirements of attending an orientation meeting and submitting an application. In Washington, four percent of targeted UI claimants completed the

initial requirements and were eligible for SEED participation; in Massachusetts, an even smaller proportion, two percent, met the same requirements and were eligible for Enterprise Project participation. Thus, relatively few new UI claimants choose to pursue self-employment when the opportunity arises.

The main findings from our analysis of program implementation are:

- In Washington, 7.5 percent of the 42,350 targeted new UI claimants invited to a meeting about the SEED Demonstration attended that meeting. In Massachusetts, 4.2 percent of the 63,921 invitees attended a meeting about the Enterprise Project.

- In both demonstrations, the recruitment and intake procedures were implemented as designed, meeting the program objective of early intervention. In Washington, attendance at the information session occurred on average 18 days after the initial UI claim date; in Massachusetts this interval was on average 33 days. The longer interval in Massachusetts relative to Washington is largely due to the fact that Washington was a wage-reporting state for UI purposes and Massachusetts was a wage-request state (i.e., had to request wage data from employers to determine claimants' eligibility for UI benefits) during the demonstration. Massachusetts procedures have recently changed and both are now wage-reporting states.

- In Washington, the 1,507 claimants who applied and were randomly assigned represent 3.6 percent of the targeted UI claimants. In Massachusetts, the 1,222 applicants who were randomly assigned represent 1.9 percent of the targeted UI claimants.

- In Washington, treatment group members began training services, on average, within 6 weeks of their initial UI claim date. In Massachusetts, training services began within 9 weeks.

- In both states, a high proportion of treatment group members participated in program services such as business training and counseling. In Washington, for example, approximately 85 percent of the treatment group attended the first training session and 70 percent received some counseling. In Massachusetts, the proportions were even higher.

- Program design differences led to greater utilization of counseling services in Massachusetts than in Washington. The mean hours of counseling in Massachusetts was 7.5 hours per participant, while the mean number of hours in Washington was 1.5.

- In both states, treatment group members received regular benefit payments while developing their businesses. Additionally, in Washington, treatment group members were eligible for a lump-sum payment (equal to their remaining available UI benefits) after achieving five program milestones. A total of 451 treatment group members (60 percent) received a lump-sum payment averaging $4,225. There was no lump-sum payment in Massachusetts.

Data Sources

Both the Washington and Massachusetts analyses are largely based on data from the Participant Tracking System (PTS), an on-line database system developed by DOL to provide information about project participants and project services, and two telephone surveys. The first Washington telephone followup survey was conducted, on average, 21 months after random assignment; a total of 1,204 sample members (or 80%) responded to this survey. In Massachusetts, the first telephone followup survey was conducted, on average, 19 months after random assignment; 449 sample members (or 80%) responded to this survey. The second telephone followup survey in each State was conducted approximately one year after the first survey, and approximately 90% of the Wave I respondents, responded in each State. In Washington, the second followup survey was conducted, on average, 33 months after random assignment; in Massachusetts, 31 months after random assignment.

Demonstration Results

In the full report we present impact results for two observation periods: the period from random assignment to the first followup survey, and the period from random assignment to the second followup survey. For simplicity, the text below summarizes only the results for the longer observation period (approximately 31 months in Massachusetts and 33 months in Washington).

Self-Employment Impacts. The Washington SEED program had a very large and positive impact on the self-employment experiences of UI claimants. Specifically, treatment group members were estimated to be much more likely than controls to have a self-employment experience, to spend more time per year in self-employment, and were more likely to be still self-employed at the time of the second followup survey. Consistent with these results, we

found that the SEED program increased claimants' self-employment earnings by over $1,600 per year.

In contrast, The Massachusetts Enterprise Project had significant positive impacts on only some of the self-employment outcomes analyzed. Specifically, treatment group members were more likely than controls to have a self-employment experience and to spend more time per year in self-employment. In contrast to the Washington demonstration, treatment group members were not significantly more likely than controls to be self-employed at the time of the second survey; furthermore, the Massachusetts demonstration did not have a significant impact on self-employment earnings.

Self-Employment Impacts

	Washington	Massachusetts
Percent Self-Employed Since Random Assignment	+22%***	+12%**
Time Self-Employed Per Year	+2.0*** months	+0.8* months
Percent Self-Employed at Second Survey	+12%***	+5%
Self-Employment Earnings Per Year	+$1,675**	+$1,219

***Indicates coefficient is significantly different from zero at the .01 level.
**Indicates coefficient is significantly different from zero at the .05 level.
*Indicates coefficient is significantly different from zero at the .10 level.

Wage and Salary Outcomes. In Washington, the estimated program impacts on wage and salary employment measures were consistently negative. Specifically, SEED significantly reduced claimants' likelihood of working in wage and salary employment, reduced number of months per year working in wage and salary employment, and reduced earnings from wage and salary employment by about $1,800 per year.

In contrast to the Washington results, the Massachusetts estimated impacts on wage and salary outcomes were generally insignificant; only the impact on earnings was significant. Indeed, the Enterprise Project significantly increased claimants' earnings from wage and salary employment by over $3,000 per year. This positive earnings impact contrasts dramatically with the negative Washington impact on earnings from wage and salary employment.

Wage and Salary Impacts

	Washington	Massachusetts
Percent Employed Since Random Assignment	-6%***	-4%
Time Employed Per Year	-0.7*** months	+0.6 months
Percent Employed at Second Survey	-3%*	+1%
Wage & Salary Earnings Per Year	-$1,780**	+$3,053**

***Indicates coefficient is significantly different from zero at the .01 level.
**Indicates coefficient is significantly different from zero at the .05 level.
*Indicates coefficient is significantly different from zero at the .10 level.

Combined Self-Employment and Wage and Salary Outcomes. The Washington demonstration did not significantly increase the combined likelihood of employment in either a wage and salary job or in self-employment. The program did, however, increase the time worked per year and increased the likelihood of being employed at the time of the second survey. On the other hand, the impacts on total earnings (self-employment and wage and salary earnings) were not statistically different from zero. Thus, it appears that SEED's positive impact on self-employment earnings together with its negative impact on wage and salary earnings resulted in a zero impact on claimants' total earnings.

In contrast, the effect of the Massachusetts Enterprise Project on total employment and earnings outcomes was large and consistent. Specifically, the program significantly increased

the likelihood of finding employment, increased the time worked per year, and increased the likelihood of being employed at the time of the second survey. Perhaps the greatest impact, however, was on total earnings. The demonstration increased combined annual earnings by nearly $6,000. Thus, it appears that the Enterprise Project had a dramatic positive impact on claimants' total earnings.

Combined Self-Employment and Wage and Salary Impacts

	Washington	Massachusetts
Percent Employed Since Random Assignment	+0%	+5%**
Time in Employment Since Random Assignment	+1.1*** months	+1.9*** months
Percent Employed at Second Survey	+6%**	+6%*
Total Earnings Since Random Assignment	+$289	+$5,940***

***Indicates coefficient is significantly different from zero at the .01 level.
**Indicates coefficient is significantly different from zero at the .05 level.
*Indicates coefficient is significantly different from zero at the .10 level.

Nonparticipant Employment Outcomes. The Washington demonstration had a significant positive impact on the employment of nonparticipants (family and nonfamily employees, excluding the business owner(s)) during the 33-month observation period. Specifically, we estimate that SEED businesses created a total of 316 nonparticipant jobs; control group businesses, created 128 jobs. The difference of 188 nonparticipant jobs may be attributed to the Washington demonstration. In contrast, the Massachusetts demonstration did not have a significant impact on nonparticipant job creation.

Benefit Payment Outcomes. The Washington demonstration significantly increased receipt of total benefits. Taking into account both the regular UI benefits and the lump-sum

payment provided to treatment group members who met the required milestones for starting a business, SEED increased total benefits by approximately $1,000. In contrast to the Washington results, the Massachusetts demonstration significantly reduced receipt of total benefits by nearly $900.

Cost-Benefit Analysis

The Massachusetts Enterprise Project generated large net benefits from each of several perspectives examined. From the perspective of participants, the net benefits during the 31-month observation period was over $11,000; from nonparticipants' perspective, the net benefit was over $2,000; and, from society's perspective, the net benefit was over $13,000. An examination of the benefits and costs from government's perspective also indicated that the Massachusetts Enterprise Project had large (over $2,000) net benefits. Given these strong results, the Massachusetts Enterprise Project appears to be a highly cost-effective policy tool for assisting UI claimants who are interested in pursuing self-employment.

The Washington SEED program had positive net benefits from participants' perspective (approximately $2,000) and from society's perspective (approximately $700). From the nonparticipant (i.e., taxpayer) and government perspectives, however, there were net costs of over $1,200. Given these results, the Washington program, like the Massachusetts program, is cost-effective from participants' and from society's perspectives. However, in contrast to the Massachusetts program, the Washington program has significant redistributional implications and net costs to the government.

CONCLUSIONS

The results of this study indicate that both the Washington and Massachusetts demonstrations increased the likelihood of self-employment, both increased the total time in employment, and both reduced the length of unemployment. Moreover, both programs are cost-effective from society's perspective; however, only the Massachusetts program is cost-effective from the nonparticipant and government perspectives. These results indicate that SEA is a cost-

effective approach to promote the rapid reemployment of unemployed workers and should be permanently incorporated into the U.S. employment security and economic development system.

1

INTRODUCTION

On December 8, 1993, President Clinton signed the North American Free Trade Agreement (NAFTA) Implementation Act (P.L.103-182). Section 507 of the Act authorizes states to establish self-employment assistance (SEA) programs as part of the state unemployment compensation system. To establish SEA programs, states must enact legislation to conform to the Federal legislation. As of December 1994, five states have enacted such legislation: California, Connecticut, Maine, New York, and Rhode Island.

The provisions of the Act give states the ability to add self-employment programs to the current mix of reemployment policy tools available to help unemployed workers. Specifically, the Act allows states to pay a self-employment allowance in lieu of unemployment compensation to unemployed workers while they are establishing businesses and becoming self-employed. To qualify for these self-employment allowance payments, program participants must receive business counseling, attend entrepreneurial training courses, and receive other technical assistance services in support of their self-employment effort.

The main parameters of this new self-employment assistance program are based on two experimental demonstrations sponsored by the U.S., Department of Labor (DOL). The Washington State and Massachusetts Unemployment Insurance Self-Employment Demonstrations were initiated by DOL to test the ability of the U.S. employment security and economic development systems to help Unemployment Insurance (UI) recipients start businesses. These demonstrations provided selected UI claimants with business development assistance, in the form of entrepreneurial training, business support services and financial assistance.

Similar self-employment programs for the unemployed have been implemented in a number of Western European countries, including Belgium, Denmark, Finland, France, Germany, Ireland, Italy, Luxembourg, the Netherlands, Norway, Portugal, Spain, and Sweden. The two largest programs are in France and Great Britain. The French Chomeurs Createures ("Unemployed Entrepreneurs") provides participants with a single lump-sum payment for

business start-up capital. The British Enterprise Allowance Scheme provides participants with a weekly self-employment allowance in lieu of their regular unemployment benefits. In a number of respects, the Washington Demonstration resembles the French program; similarly, the Massachusetts Demonstration resembles the British program. Thus, the UI Self-Employment Demonstrations test two alternative models of self-employment assistance programs.

BACKGROUND OF THE UI SELF-EMPLOYMENT DEMONSTRATIONS

Traditionally, the unemployment compensation system has provided income support for workers who are temporarily laid off or expect to be unemployed for only a short time. In recent years, however, an increasing number of workers have permanently lost their jobs. These displaced workers require more than temporary income maintenance to re-enter the workforce. For some, self-employment may be the best path to re-enter the workforce.

Over the past decade, the U.S. Department of Labor (DOL) has launched a series of experimental demonstrations to investigate alternative reemployment strategies. The strategies tested in these demonstrations have included job search assistance, occupational retraining, relocation assistance, and reemployment bonuses (monetary incentives for early re-employment in new jobs). In addition to these demonstrations that focused on reemployment into wage and salary jobs, DOL also initiated experimental demonstrations to test the efficacy of self-employment assistance as a reemployment strategy.

In the late 1980's, DOL initiated the Washington State and Massachusetts UI Self-Employment Demonstrations to test the ability of the employment security and economic development systems to help UI recipients start businesses. Both of these demonstrations provided participants with business development assistance, in the form of entrepreneurial training, business support services, and financial assistance. A brief description of each of the demonstrations follows.

WASHINGTON SELF-EMPLOYMENT AND ENTERPRISE DEVELOPMENT (SEED) DEMONSTRATION

The first federally-sponsored project in the U.S. to test the use of self-employment programs as a reemployment strategy was the Washington Self-Employment and Enterprise Development (SEED) Demonstration. The SEED Demonstration was initiated on a pilot basis in one site beginning in September 1989 and was then implemented in five additional sites in February 1990. Demonstration intake activities continued through early September 1990, with business support services available to demonstration participants through March 1991.

To allow rigorous evaluation of program effectiveness, the SEED Demonstration used a classical experimental design with random assignment of eligible claimants interested in starting their own businesses. These individuals were randomly assigned to either a treatment group, that was eligible to receive all program services, or to a control group, that was not eligible to receive program services, but remained eligible for regular UI benefits. Using this experimental design, the impacts of program services can be measured directly by the difference in outcomes between the treatment and control groups. A total of 755 new claimants were enrolled in SEED in the six sites and offered demonstration services; 752 new claimants who applied to SEED were assigned to the control group.

THE MASSACHUSETTS UI SELF-EMPLOYMENT DEMONSTRATION (THE ENTERPRISE PROJECT)

A second self-employment demonstration was mandated by Section 9152 of the Omnibus Budget Reconciliation Act (OBRA) of 1987. This Act authorized up to three States to participate in a self-employment experimental demonstration. In 1988, Massachusetts agreed to participate in the demonstration. In designing the Massachusetts UI Self-Employment Demonstration (Enterprise Project), the Department of Labor, the State of Massachusetts, and the researchers (Abt Associates/Battelle) were guided by two objectives. The first was to develop a program, consistent with the authorizing legislation, designed to facilitate self-employment for UI claimants who choose this avenue. The second objective was to develop a design that would permit a scientifically valid program evaluation.

The provisions of the authorizing legislation mandated a number of important demonstration design features. For example, the Act required that the demonstration target self-employment services to UI claimants "likely to receive regular or extended benefits for the maximum number of weeks that such compensation is made available under the State law during such benefit year" (Section 9152(i)). Another important provision of the legislation was to require participating states to reimburse the Unemployment Trust Fund for any excess costs incurred as a result of the demonstration (Section 9152(c)). Excess costs arise when demonstration treatment group members, on average, collect more self-employment allowances than the amount of UI benefits they would have collected in the absence of the demonstration (as measured by the experience of control group members). Finally, the authorizing legislation required the demonstration to be implemented over a three-year period.

The Massachusetts Enterprise Project began operations in May 1990. Enrollment into the Enterprise Project took place during three distinct phases. The first enrollment phase took place in 1990 (May - September), the second in 1991 (April - October), and the third in 1992-93 (March 92 - April 93). As in the Washington Demonstration, the Massachusetts Demonstration used a classical experimental design with random assignment of eligible claimants interested in starting their own businesses. A total of 1,222 UI claimants were randomly assigned to either the treatment group (614) or the control group (608) during all three enrollment periods.[1]

Organization of the Report

In this report we present the final impact results and the cost-effectiveness analysis of the Washington and Massachusetts Demonstrations. Previous reports have described the implementation and early impacts of the two demonstrations. Specifically, a complete description of the SEED Demonstration implementation, is found in Johnson and Leonard (1991); the early implementation of the Enterprise Project is described in Benus et al. (1990); the first impact results of the SEED Demonstration are described in Benus, Johnson et al.

[1] In 1990, 207 UI claimants were randomly assigned; in 1991, 314 were randomly assigned, and in 1992-93, 701 were randomly assigned.

(1993); finally, a comparison of the early impacts of the SEED Demonstration and the early impacts of the Enterprise Project is presented in Benus et al. (1994).

In this report, we present the final impact results of the UI Self-Employment Demonstrations. These impact results are based on administrative data collected throughout the three-year demonstration observation period as well as on survey data collected in two follow-up surveys. The first survey was conducted approximately 20 months after random assignment and the second survey was conducted approximately 33 months after random assignment.

This report also presents, for the first time, information on the cost-effectiveness of the two self-employment assistance programs that were tested as part of the UI Self-Employment Demonstrations. The cost-effectiveness results are based on a comparison of program impacts and program costs. For program impacts, we use the 33-month impact estimates derived in Chapters 7 and 8. Program costs are derived from data provided by program operators. By comparing program benefits with program costs, we can evaluate if the programs tested are cost-effective.

This report is organized in three main parts. Part I (Chapter 1 through Chapter 6) gives an overview of the UI Self-Employment Demonstrations, the implementation experiences, and the data used in the analysis.

The details of the experimental and operational design of the two demonstrations are presented in Chapter 2. For each demonstration, we describe the design of the targeting criteria, the recruitment and intake process, the application and random assignment procedures, the program services, and the financial assistance provided.

In Chapter 3, we describe the implementation of the Washington Demonstration. Since the implementation of SEED has been described in detail in Johnson and Leonard (1991), we only present a brief summary of the SEED implementation experiences. We summarize demonstration intake results, business support services, and financial assistance provided to SEED participants.

In Chapter 4, we describe the implementation of the Massachusetts Enterprise Project in greater detail. We first note demonstration intake results and the characteristics of Enterprise Project participants. Next, we look at the main changes in program implementation procedures.

We also analyze participation in demonstration activities, the timing of these activities, and the changes in UI regulations that were implemented during the course of the demonstration.

In Chapter 5, we compare the main implementation results of the Washington and Massachusetts Demonstrations. By comparing program experiences, we gain insights about the impact differences that are found in later chapters.

Chapter 6, the last chapter in Part I, provides a description of the data sources used in the impact and cost-effectiveness analysis. We first describe the Participant Tracking System (PTS), an on-line database system developed by DOL that provides data on personal characteristics, demonstration services, business information, and UI benefits information. We then describe the administrative records data, including the State UI Wage Records. Finally, we describe the contents of the followup survey and the procedures used in its administration.

Part II of the report (Chapter 7 through Chapter 9) provides our estimates of the program impacts on key outcome measures. Chapter 7 focuses on the Washington Demonstration, Chapter 8 focuses on the Massachusetts Demonstration, and Chapter 9 compares the impact estimates from the two demonstrations.

We begin Chapter 7 by describing the characteristics of Washington participants who entered self-employment during the observation period. Following this descriptive analysis, we evaluate SEED program impacts on the likelihood of being self-employed, time in self-employment, and earnings from self-employment. We also investigate other demonstration impacts, including the impacts on UI outcomes.

In Chapter 8, we present an analysis of the impacts of the Massachusetts Enterprise Project. As in the SEED analysis, we investigate self-employment outcomes, wage and salary outcomes, combined (self-employment or wage and salary) outcomes, and other outcomes.

In Chapter 9, we compare the demonstrations' impacts. We also investigate the potential impact of "profiling" (i.e., targeting claimants who are likely to exhaust benefits) on the observed impact results. This latter issue is of particular interest to policymakers since States must use profiling in implementing their self-employment assistance programs.

Part III, which includes only Chapter 10, presents our analysis of the cost-effectiveness of the two demonstrations.

2

EXPERIMENTAL AND OPERATIONAL DESIGNS

In this chapter we compare and contrast the experimental and operational design features of the Washington and Massachusetts demonstrations. We focus on the features of the demonstrations that are important for understanding and interpreting the impact results that will be presented later in this report.

As an aid for understanding the two experimental designs, we present in Exhibit 2.1 a flow chart depicting the intake and random assignment procedures used in each of the demonstrations. As indicated by the exhibit, the overall flow is similar in both demonstrations. The main differences in experimental design are discussed in depth in the following subsections.

TARGETING DEMONSTRATION PARTICIPANTS

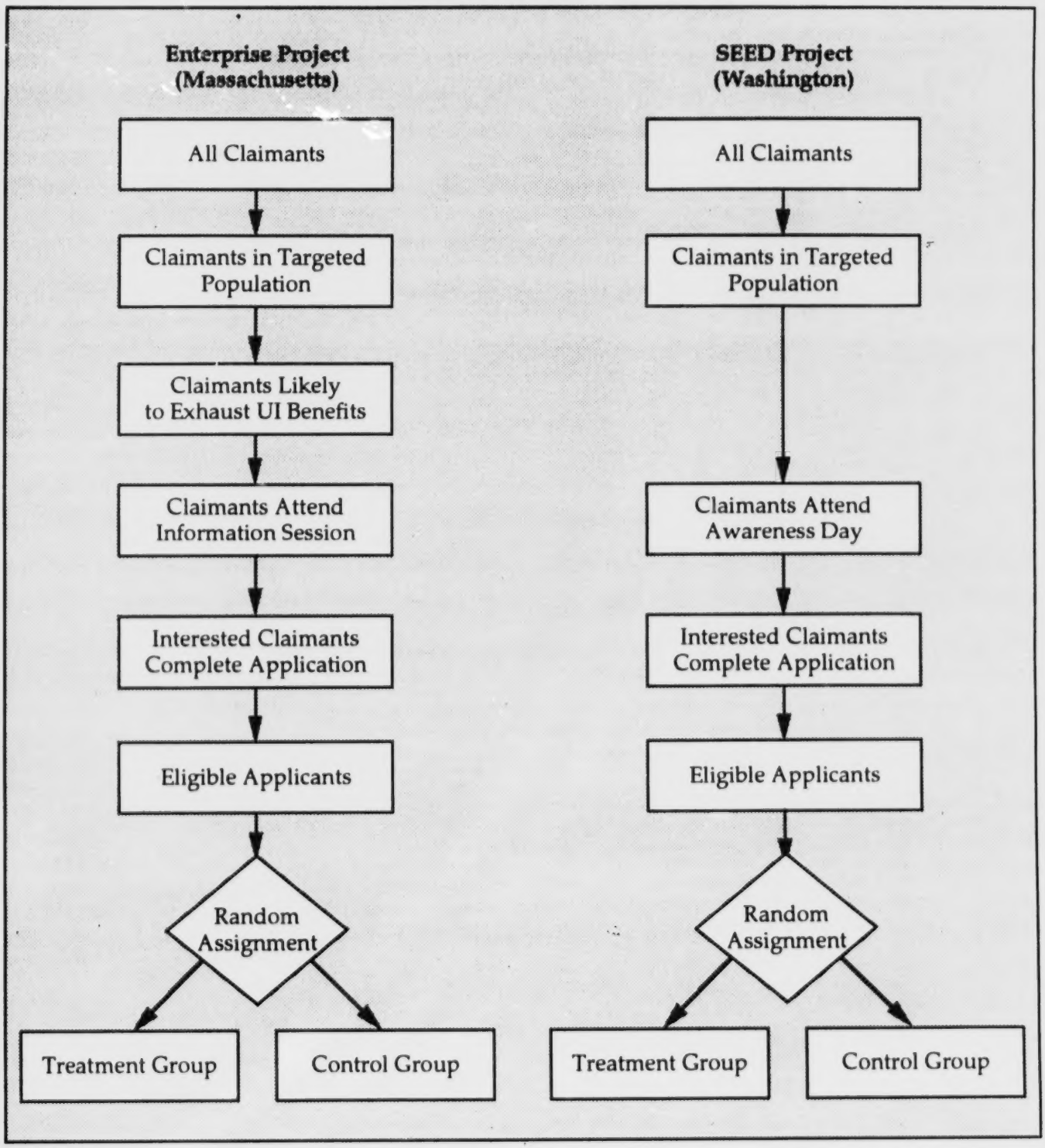
Both the Massachusetts and the Washington projects targeted new UI claimants. In both states, the following claimants were excluded:

- Persons filing interstate claims;
- Claimants who were employer-attached (i.e., on standby); and,
- Claimants under 18 years of age.

In addition, the Washington demonstration excluded persons who were full-referral union members and those filing claims backdated more than 14 days; the Massachusetts demonstration excluded claimants eligible for less than 26 weeks of UI benefits.

Since the authorizing legislation for the Massachusetts demonstration required that the program focus on UI claimants who were likely to exhaust their UI benefits, further targeting was necessary to select those claimants likely to exhaust benefits. To implement this legislative requirement, the sample selection was based on an algorithm that predicted each claimant's

EXHIBIT 2.1
ENTERPRISE PROJECT AND SEED PROJECT INTAKE AND ASSIGNMENT PROCESSES



likelihood of UI benefit exhaustion.[2] Using this algorithm, a numerical probability of exhaustion was calculated for each new Massachusetts claimant in the target population. Entry into the demonstration was then restricted to those with a high predicted probability of exhausting UI benefits.[3]

Recruiting Participants

The next step in both demonstrations was to recruit claimants interested in self-employment. Once identified, eligible claimants were sent a letter inviting them to attend an initial information session. To ensure that the most highly-motivated claimants were identified for the self-employment program, strict time limits were established for attending this session. Only claimants who met these time constraints were permitted to continue in the demonstration. In this way, self-screening eliminated the less-motivated claimants from the demonstration.

In both demonstrations, the information sessions were held in the local UI office in which the claimant filed his or her claim.[4] The key difference between the Washington and Massachusetts information sessions was in the format of the presentations. In Washington, a local UI office staff person took attendance, introduced a set of two videos (covering the key features of SEED and the risks and rewards of self-employment), showed the videos, and answered questions at the end of the session. In Massachusetts, on the other hand, the presentations were given by a local UI office staff member and a business development expert. The local UI staff member described the demonstration procedures and distributed applications at the end of the session; the business development experts gave a presentation on the risks and rewards of self-employment.

---

[2] For details on the algorithm, see Benus, et al. "Massachusetts UI Self-Employment Demonstration Interim Report to Congress" in *Self-Employment Programs for Unemployed Workers*, U.S. Department of Labor, Employment and Training Administration, 1992.

[3] In addition, to regulate the flow of claimants into both demonstrations, further subsampling occurred in the largest demonstration sites.

[4] In Massachusetts, the schedule for the sessions was weekly during year one. The schedule was modified after the first year and information sessions were conducted on a bi-weekly basis in the three sites with the lowest number of new UI claimants. In Washington, information sessions were conducted bi-weekly (every-other Friday) except during the pilot in Vancouver when the sessions were held weekly.

The purpose of the initial information session was to provide claimants with sufficient information about the self-employment program to decide whether or not to apply for program services. During the session, claimants were provided with basic information about the risks and rewards of self-employment and the key features of the demonstration. The Massachusetts sessions lasted approximately 60 minutes; the Washington sessions were scheduled to last approximately 30 to 45 minutes.

APPLICATION AND RANDOM ASSIGNMENT

At the conclusion of the initial information sessions, claimants who were still interested in applying to the program took application materials home to complete. Applicants were required to return the completed materials within seven days. The completed applications contained personal background information and a description of the applicant's proposed business idea.

The applications were reviewed by project staff for timeliness and completeness. The business ideas were also reviewed to ensure that they conformed to established project guidelines.[5] The business idea was not evaluated on its merit or on its likelihood of success. Those applicants who submitted the applications on time and satisfied the project guidelines were eligible for random assignment. Eligible applicants were then randomly assigned to either the treatment group that was eligible to receive business development services and financial assistance or to a control group that was not. Control group members were informed that they were not selected for the experimental program and that they must continue to meet all UI eligibility requirements. Both treatment and control group members were informed that they would be contacted in the future for research purposes.

[5] For example, the business idea must be legal and the participant must have day-to-day control of the business. It is important to note that the business idea itself was not evaluated on its likelihood of success. In other words, we did not attempt to identify "winners" for inclusion in the demonstration.

BUSINESS START-UP SERVICES AND FINANCIAL ASSISTANCE

The flow chart in Exhibit 2.2 highlights the differences in business start-up services in the two demonstrations. We summarize these below.

Massachusetts

Once they were assigned to the treatment group, the first step for Massachusetts participants was to attend an Enterprise Seminar, a one-day session that was conducted by one or more business experts.[6] This intensive training session followed a standard curriculum and covered topics such as developing a business mindset, business organizational structures, marketing, business plan development, personnel issues, and business management. Within two weeks of the Enterprise Seminar, participants were required to attend an individual counseling session with their business counselor. This counseling session lasted approximately one hour. In addition to this required counseling session, participants were encouraged to schedule additional counseling sessions with their business counselors as needed. Massachusetts participants were also required to attend Enterprise Workshops, six two-hour group sessions focused on the following topics:[7]

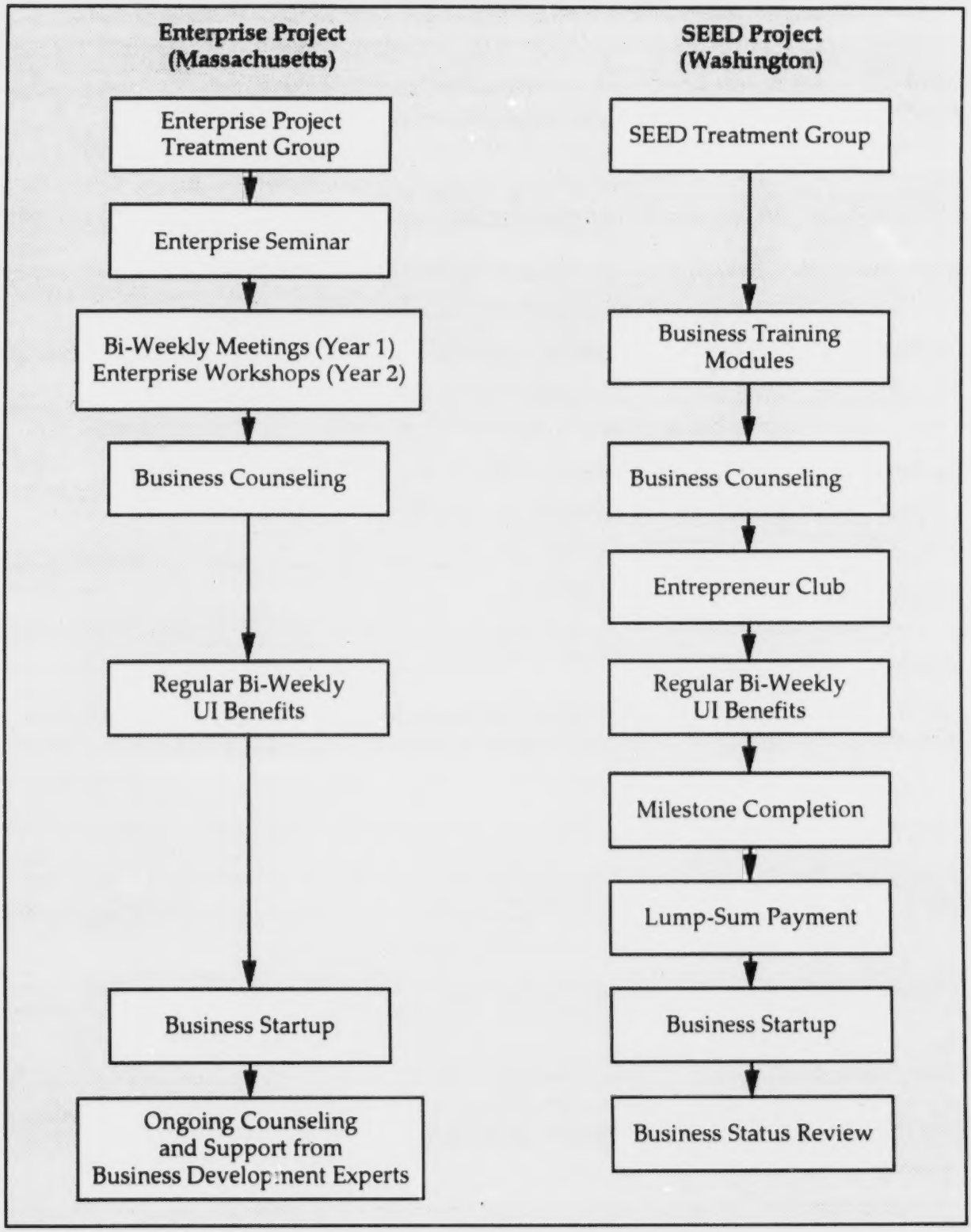
- Marketing;
- Personal effectiveness and selling;
- Cash flow;
- Financing;
- Legal requirements and insurance; and,
- Bookkeeping/taxes.

These six sessions were offered over approximately a twelve-week period during the first and second years. During the third year, the schedule for the sessions was streamlined, so that all

[6] In 1990, this session was eight hours long. Program administrators determined that eight hours was too long and difficult for participants. As a result, in 1991, the session was shortened to 4½ hours (in 1992 it was lengthened to 5½ hours).

[7] During 1990, the sessions were called Bi-Weekly Meetings and did not follow a structured set of topics. In 1991, a more structured schedule of topics was developed for these sessions. In 1992, the sessions were extended to 2 1/2 hours in length and participants were required to give 15-minute presentations of their business plans.

EXHIBIT 2.2
BUSINESS STARTUP SERVICES



sessions could be completed over a nine week period. During this period, participants were encouraged to develop a business plan with the assistance of their counselors.

The financial assistance component in Massachusetts included payment of regular bi-weekly UI benefits, with an exemption from the regular UI work search requirements while in the demonstration. The demonstration design set the duration of the UI work search waiver at 24 weeks. Thus, in Massachusetts, treatment group members could collect self-employment allowances through the 24th consecutive week of their UI claim. Since Massachusetts claimants were eligible for up to 30 weeks of UI benefits, at the 24th week they were required to choose between continuing with their self-employment activities full-time or returning to UI for the remaining six weeks of UI eligibility and meeting the work search requirements.[8]

To provide additional financial support, the Enterprise Project developed a loan program through Shawmut Bank, a large regional bank with branches in each of the demonstration sites. As part of this program, participants' loan applications were given consideration, even if the size of the loan fell below normal minimum levels.

Washington

The business start-up services provided in the Washington demonstration differed from the services provided in the Massachusetts demonstration. Within two weeks of random assignment, treatment group members were scheduled to attend a set of four business training modules covering the following topics:

- Business feasibility;
- Marketing;
- Finance and accounting; and,
- Organization and management.

[8] During the third year of implementation (1992-1993), slightly different rules were in effect regarding UI eligibility. During this period, when Extended Benefits were available, Massachusetts UI claimants were eligible for only 26 weeks of regular benefits. That is, Massachusetts passed a law which stated that all claimants filing during the time when Extended Benefits were in affect would be eligible for 26 weeks of regular UI benefits and then would become eligible for Federal Extended Benefits. This change in law did not affect the total amount of benefits claimants could receive, but did affect the amount of benefits paid from the Massachusetts UI Trust Fund.

It took approximately 20 hours of classroom time to cover these four topics; the Massachusetts classroom sessions (i.e., the Enterprise Seminar and six Enterprise Workshops) took approximately the same classroom time to complete. The four Washington training modules were presented in four sessions during a one-week period; in contrast, the seven Massachusetts sessions were presented over a 12-week period.

The training modules introduced claimants to the need for developing a comprehensive business plan. Individualized business plans were then developed by participants with the assistance of a business development specialist. Additional assistance in developing a business plan was offered through Entrepreneur Club meetings, which were scheduled monthly. These meetings provided participants with peer support and advice throughout their demonstration participation.

The financial assistance component of the Washington demonstration included regular bi-weekly UI payments while participants were engaged in business start-up activities as well as eligibility for a lump-sum payment of the remaining available UI benefits when they completed the following five milestones:

- Completion of the training sessions;
- Development of an acceptable business plan;
- Establishment of a business bank account;
- Satisfaction of all licensing requirements; and,
- Obtaining adequate financing.

Following the completion of these milestones, participants were eligible for a lump-sum payment equal to their remaining UI entitlement at the time. Because the remaining entitlement at any point in the claim is the maximum benefits payable less the amount of UI benefits already paid out, the amount of the lump-sum payment depended on the participant's UI entitlement, as well as the time taken to achieve the milestones.

Although the lump-sum payment component of the SEED Demonstration was intended to simulate a cash-out of UI benefits, it was not strictly possible to test a cash-out policy because UI is an entitlement program that could not be denied for demonstration purposes. Operationally, this meant that participants could return to the regular UI program after receiving their lump-sum payment and draw the remainder of their UI entitlement in the form of bi-weekly

payments provided they met the normal UI eligibility requirements, including the work search requirement.[9] Nonetheless, most claimants thought it was a cash-out of their UI benefits and very few returned to UI.

One key element in the Washington demonstration design was the role played by the business development specialists. These specialists provided ongoing counseling, assisting each participant in his/her pursuit of the five program milestones that were required to receive a lump-sum payment. The business development specialists were also responsible for conducting a "milestone review" to determine if all milestones had been attained. After the business start-up, the business specialists' responsibilities included technical assistance on an as-needed basis and a business status review, conducted approximately two months following receipt of the lump-sum payment.

Participation Requirements

Each demonstration required participants to complete certain activities within a specified timeframe. Other activities were considered optional. In Washington, participants were required to attend the four training modules (or ask to have them waived). Counseling sessions were available to participants who wanted them, but were not required. Entrepreneur Club meetings were an optional activity.

In Massachusetts, participants were required to attend the Enterprise seminar, one counseling session, and six Enterprise workshops. Additional counseling sessions were optional.

[9] Because the lump-sum payments were paid out of Federal research funds -- not State UI funds -- they did not affect a participant's UI net balance available.

3

WASHINGTON DEMONSTRATION IMPLEMENTATION

In this chapter, we review the implementation of the Washington Self-Employment and Enterprise Development (SEED) Demonstration. A more complete description of the SEED demonstration implementation may be found in Johnson and Leonard (1991). Here, we present only those attributes of the implementation that are important for understanding the impact analysis that will be presented in later chapters.

In the following sections, we provide a brief overview of demonstration implementation experiences. We first describe the flow of claimants through the demonstration intake process — from the identification of targeted claimants through random assignment. This includes evidence on the comparability of the treatment and control groups, as well as the timing of intake activities. We then describe the business support services and financial assistance received by treatment group members from the SEED Demonstration.

DEMONSTRATION INTAKE

The SEED Demonstration was implemented on a pilot basis in one site (Vancouver) in September 1989, and in five additional sites beginning in February 1990.[10] Demonstration intake activities continued through September 1990, with business support services available to demonstration participants through March 1991.

Development of Experimental Sample

As described in Chapter 2, the development of an appropriate experimental sample of SEED Demonstration participants involved several steps. The first step was to identify targeted

[10] Because only minor changes were made prior to full implementation, we combine the data for the relatively few claimants who were part of the pilot study with the data for the full demonstration sample.

new claimants without immediate job prospects and invite those interested in self-employment to attend an initial information session, called Awareness Day in the SEED Demonstration. A total of 42,350 invitation letters were sent to targeted claimants in the six sites during the intake period.[11] The six sites in the demonstration were: Vancouver, Olympia, King County, Snohomish County, Wenatchee, and Yakima. The location of these demonstration sites is depicted in Exhibit 3.1.

In Table 3.1, we provide information on the characteristics of new UI claimants in the six demonstration sites. Additionally, the characteristics of targeted claimants are compared with the characteristics of those who were excluded from the target group and not invited to participate. These results are based on Participant Tracking System (PTS) data[12] and Unemployment Insurance (UI) wage records for a 10 percent random sample of new claimants who filed for UI benefits in the six sites during the demonstration intake period. As Table 3.1 indicates, about 62 percent of all new claimants in the six sites were male, 84 percent were white, and 30 percent attended or completed college. The mean age was 36 years, with 22 percent being at least age 45 or older. The mean earnings in covered employment during the four calendar quarters prior to the quarter of filing for benefits was just under $15,000. The mean weekly UI benefit amount (WBA) for all new claimants was $152, with a mean maximum benefits payable of $4,002.

The results in Table 3.1 clearly indicate that targeted claimants were quite different from non-targeted claimants in ways that could be expected given the targeting criteria. In particular, non-targeted claimants were much more likely to be union members and on standby. As a result, non-targeted claimants were much less likely to be in professional, technical, or managerial occupations or in clerical occupations. They were more likely than targeted claimants to be male, white, and slightly older, and were less likely to have any post-secondary education. In addition, non-targeted claimants had considerably higher earnings in the prior year, and correspondingly higher average weekly benefit amount and maximum benefits payable.

---

[11] The two primary reasons that claimants were excluded from the SEED target population were: (1) employer attachment (50.3 percent of those excluded were on standby) and, (2) the claimant was a member of a full-referral union (28.9 percent). Only 18.0 percent were excluded because they had a backdated claim and very few claimants were excluded from the SEED target group because they were under age 18 or had filed an interstate claim.

[12] The participant tracking system (PTS) and its data are described in Chapter 6.

Exhibit 3.1

Washington SEED Demonstration Sites

BEST COPY AVAILABLE

Table 3.1
Washington
Characteristics of Targeted and Non-Targeted New UI Claimants

Claimant Characteristics	Targeted (Invited to Awareness Day)	Non-Targeted (Not Invited to Awareness Day)	All New Claimants
Demographics			
Percent male	57%	80%	62%
Percent white	82%	91%	84%
Percent high school graduate	44%	56%	47%
Percent some college	22%	19%	21%
Percent college graduate	10%	4%	9%
Mean education (in years)	12%	12%	12%
Percent age $\leq$ 24	18%	12%	17%
Percent age $\geq$ 45	21%	25%	22%
Mean age (in years)	35	37	36
Prior Work Experience			
Percent union hiring hall member	0%	26%	6%
Percent on standby	0%	55%	12%
Percent professional/technical/ managerial occupation	15%	4%	13%
Percent clerical occupation	16%	5%	13%
Percent manufacturing sector	22%	15%	21%
Percent services sector	24%	27%	25%
UI Wages in prior year ($)	$13,743	$19,270	$14,901
UI Entitlement			
Mean weekly benefit amount ($)	$144	$183	$152
Mean maximum benefits payable ($)	$3,737	$5,003	$4,002
Site			
Percent in Vancouver	24%	39%	27%
Percent in Olympia	12%	20%	14%
Percent in King County	26%	20%	25%
Percent in Snohomish County	10%	11%	10%
Percent in Wenatchee	14%	3%	12%
Percent in Yakima	14%	7%	12%

The second step in the intake process involved recruiting targeted claimants for the demonstration. Of the 42,350 targeted new claimants who received an invitation to attend an Awareness Day meeting, 3,167 (7.5 percent) were interested enough in the possibility of participating in the self-employment program to attend. The take-up rate differed somewhat by site, from a low of 4.6 percent in Wenatchee to a high of 9.8 percent in Snohomish. Overall, the take-up rates were lower in the rural areas of Wenatchee and Yakima. These low rates in the Wenatchee and Yakima sites relative to other sites may be indicative of a lower interest in self-employment among targeted new UI claimants in rural areas than in urban areas. Alternatively, it may reflect the much higher unemployment rates in these rural areas and represent claimants' assessments of the prospects for self-employment in such environments.

At the end of the Awareness Day meeting, interested claimants were provided with a SEED application packet. Of the 3,167 targeted claimants who attended the Awareness Day meeting, 1,932 (61 percent) chose to submit a SEED application.[13] Combining the results of the first two intake steps -- Awareness Day and SEED application -- 4.6 percent of all targeted claimants who received an invitation letter submitted a SEED application.

The third step in the SEED intake process was the review of SEED applications. The applications were reviewed for timeliness (they were required to be postmarked within seven days of Awareness Day) and for being substantively complete. This review process resulted in rejecting very few applicants at this stage as the applications were generally quite detailed and of high quality. Specifically, of all applications submitted, only 52 (2.7 percent) were rejected because they were submitted late and just 20 (1.0 percent) were rejected because they were not complete.

Among those with valid applications, the only remaining reason for exclusion from the random assignment pool relates to UI eligibility. In particular, the primary reason for application rejection was nonmonetary ineligibility at the time of random assignment; 285 claimants who submitted applications were determined to be nonmonetarily ineligible for UI

[13] Although we have no detailed information on the reasons for why claimants who attended Awareness Day did not submit applications, limited information from the pilot study suggests that the Awareness Day meeting and the application served as a useful self-screening function. Specifically, the reasons given by a small sample of claimants who attended the meeting during the pilot study but who did not submit an application were primarily related to lack of adequate capital or concern over whether self-employment was right given their situation.

benefits. Another 55 applicants were rejected because the claim was not monetarily valid and a few others were rejected because the UI claim was canceled.

As a result of these exclusions, a total of 425 of the 1,932 SEED applications submitted were excluded from the pool for random assignment, leaving a final pool of 1,507 claimants. The random assignment pool corresponds to 47.6 percent of all individuals who attended Awareness Day and 3.6 percent of all claimants in the target group. Over the course of the demonstration, a total of 755 claimants were randomly assigned to the treatment group and 752 to the control group.

A critical evaluation issue concerns the comparability of the individuals in the treatment and control groups. We examined the characteristics of these groups in an earlier report (Benus, Johnson, and Wood, 1993) and the results indicate that the random assignment process was very successful in generating two groups that were quite similar on all of the standard characteristics collected at the time the claim was filed. In addition, the groups are extremely similar on prior earnings, as well as key items obtained on the SEED application, including prior business experience, marital status, family status, reason for job separation, assets, and liabilities. We conducted t-tests of differences in means on these characteristics; none of the differences was statistically significant at the .05 level.

Timing of Intake Activities

The SEED Demonstration was intended to be an early intervention program. It was anticipated that by recruiting claimants for SEED as early as possible in their claim and by providing services early, the program would provide the maximum possible support to individuals during the business startup period. The program was designed to select individuals into the treatment group by the fourth or fifth week of the claim and to provide business training to treatment group members by the fifth or sixth week. Below we briefly present data that show how these timing objectives were achieved in the SEED Demonstration.

Data on the timing of intake activities through assignment to the first service -- business training modules -- are summarized overall and by site in Table 3.2. This table shows that the intake and recruitment processes occurred as planned. For example, the average length of time from the effective date of claim (EDC) until Awareness Day was 18 days, or about 2.5 weeks.

Table 3.2
Timing of Intake Activities by Site
(Percent)

Activity	All Sites (N = 755)	Site					
		Vancouver (N = 155)	Olympia (N = 94)	King County (N = 286)	Snohomish County (N = 100)	Wenatchee (N = 58)	Yakima (N = 62)
Effective Date of Claim to Awareness Day							
12 days or less	36.9	38.1	42.6	36.0	39.0	25.9	37.1
13-25 days	47.3	52.9	40.4	47.6	46.0	51.7	40.3
26 days or more	15.8	9.0	17.0	16.4	15.0	22.4	22.6
Mean days	17.7	17.1	17.7	17.7	17.4	19.2	18.2
Awareness Day to Random Assignment							
6 days or less	29.1	22.6	35.1	32.2	32.0	29.3	21.0
7-13 days	59.1	61.9	56.4	60.5	48.0	60.3	66.1
14 days or more	11.8	15.5	8.5	7.3	20.0	10.3	12.9
Mean days	11.1	11.9	10.4	10.7	11.7	10.6	11.8
Random Assignment to First Training Module							
7 days or less	53.0	51.6	43.6	52.1	50.0	67.2	66.1
8-14 days	42.4	41.3	52.1	42.3	50.0	31.0	29.0
15 days or more	4.6	7.1	4.3	5.6	0.0	1.8	4.9
Mean days	10.2	10.5	10.5	10.2	10.4	9.4	9.4
Mean Days from EDC to First Training Module	39.0	39.5	38.6	38.6	39.5	39.2	39.4

About 37 percent of the treatment group members attended an Awareness Day within 12 days of their EDC and another 47 percent attended Awareness Day between 13 and 25 days after their EDC. Because a few claimants were re-scheduled for a later Awareness Day and some meetings were postponed because of holidays, a relatively small percentage (16 percent) did not attend a meeting until about 4 weeks after their EDC. Moreover, there were relatively small differences in timing across sites.

The time from Awareness Day to random assignment took another 11 days on average. Thus, individuals in the treatment group were randomly assigned within 29 days from their EDC on average, or within about four weeks in total. There was relatively little variation across sites in the time from Awareness Day to random assignment.

Finally, in the bottom panel of Table 3.2 we show the length of time from random assignment to the scheduled date of the first training module. These data indicate that over 95 percent of the treatment group members were scheduled to attend their first business training module within two weeks of random assignment, with a mean of 10 days.

Taken together, these data indicate that the average time from EDC to the date for the first training module was 39 days or about five and one-half weeks. Thus, it appears that the timing and frequency of key intake and service activities described in Chapter 2 occurred on schedule and that the goal of early intervention was achieved.

Business Support Services

As described in Chapter 2, individuals randomly assigned to the treatment group were offered a number of business support services as well as financial assistance. The business startup services component included intensive classroom training, assistance in preparing a business plan, individual counseling, and peer support groups. The financial assistance included periodic self-employment allowance payments equal to their weekly benefit amount and a waiver of the work search requirement while they were trying to start a business. Moreover, those who met all program milestones received a lump-sum payment equal to their remaining UI entitlement.

The extent to which SEED treatment group members dropped out or completed the program and the specific services they received are important to understanding the results of the impact analysis. In the remainder of this chapter, we examine these factors and describe their SEED experiences.

Business Training Modules

Business startup assistance offered to treatment group members began with business training modules. Instructions for attending a set of four business training modules at a specific location were included in the letter sent to treatment group members informing them of their selection into SEED. As described above and in Table 3.2, the first training module was held, on average, about 10 days after random assignment. Although attendance at the first module was required — in part to ensure that the participation agreement was signed — it was possible for subsequent modules to be waived by the business development specialist (BDS) if the claimant could demonstrate proficiency in the topics covered in these modules. Treatment group members who did not attend the first training module were dropped from the demonstration.[14]

In Table 3.3, we provide summary information on business training module attendance and receipt of other SEED services. Of the 755 claimants in the treatment group, 640 (85 percent) attended the first training module. This corresponds to a 15 percent dropout rate from the treatment group prior to business training.[15] Among treatment group members who attended the first business training module very few waivers were granted and nearly all attended the remaining three modules. Thus, the attendance rate for all four modules (counting the waivers as attenders) was 83 percent of all treatment group members.[16]

---

[14] All members of the original treatment and control groups, including treatment group members who dropped out of the program, were retained in the evaluation to maintain the comparability of the treatment and control groups.

[15] The attendance rate at the first module varied across sites from 80 percent in Wenatchee, Yakima, and Olympia to 88 percent in King County.

[16] The overall module attendance rate ranged from 76 percent in Wenatchee to 87 percent in King County. The relatively high dropout rate for claimants in Wenatchee is consistent with the high proportion of claimants (over 50 percent) in that site who indicated on their SEED application that they expected to be called back to work by their previous employer.

Table 3.3
Washington
SEED Business Assistance Services Received

Business Assistance Services	All Treatment Group Members (N=755)
Business Training Modules	
Attended Module 1	85%
Attended (or Waived) All Modules	83%
Business Counseling Hours	
None	30%
.1-.9	19%
1-1.9	21%
2-2.9	13%
3-3.9	8%
4-4.9	4%
5 or more	6%
•Mean Hours of Counseling	1.5
Number of Entrepreneur Club Meetings Attended	
None	64%
1	18%
2	9%
3	5%
4 or more	5%
Mean Number of Meetings Attended	0.7

Business Counseling

In their role as case managers, business development specialists (BDSs) provided assistance to treatment group members in the form of counseling on the preparation of a business plan and on other issues. The intent was for the BDS to take a proactive role and provide individualized counseling and assistance throughout the development of the business plan and business startup. To encourage this proactive role, an objective was established for the BDSs

to make at least one follow-up contact with all participants during the first few weeks of program participation.

The second panel of Table 3.3 contains the distribution of total business counseling hours received by SEED participants. As this table indicates, 70 percent of the treatment group received some counseling, with an overall mean of 1.5 hours.[17] The results also indicate that very few participants received a substantial amount of counseling, with only 18 percent receiving 3 hours or more.[18] As described in Johnson and Leonard (1991), the major focus of the individual counseling sessions was on helping treatment group members develop a business plan. Overall, nearly one-half of the counseling activities recorded were focused on business plan development assistance.

Other Business Support Services

In addition to the business training modules and individualized counseling, the SEED Demonstration included a peer support group in each site, called the Entrepreneur Club. Beginning in the second or third month of the demonstration -- after a sufficient number of new treatment group members were available -- Entrepreneur Club meetings were scheduled on a monthly basis. As indicated in the third panel of Table 3.3, the majority of treatment group members did not take advantage of this optional peer-support group. Specifically, nearly two-thirds (64.1 percent) of all treatment group members did not attend any Entrepreneur Club meetings. Moreover, the mean number of meetings attended was 0.7. Among the 36 percent of participants who attended at least one meeting, the mean number of meetings attended was about two.

A final type of business startup service available was referral to other agencies for assistance as needed. Very few referrals to other agencies occurred during the SEED Demonstration. Specifically, administrative records suggest that there were only 43 referrals

[17] When calculated over the 630 treatment group members who completed all four training modules, the average hours of counseling increases to about 1.8.

[18] There were differences in the number of counseling hours by site. For example, about 43 percent of the treatment group members in Vancouver received at least 3 hours of counseling, as compared to none in Snohomish County, and fewer than 5 percent in Olympia or Wenatchee. Overall, it seems that there was relatively little counseling in Olympia, Wenatchee, and Snohomish County, with an average of 0.5-0.9 hours per demonstration participant.

to other agencies; 37 of these records were for the Vancouver site, and the remaining six were for claimants in King and Snohomish counties. The low frequency of this activity is consistent with the views of some BDSs that participants did not need any assistance other than that which they were receiving from SEED.

FINANCIAL ASSISTANCE

In addition to receiving their regular weekly UI benefit amount, SEED treatment group members received two other forms of financial assistance: a waiver of the work search requirement while working full-time to start a business, and a lump-sum payment equal to the remaining entitlement at the time all five milestones were met. In this section, we describe SEED Demonstration experiences with these financial assistance elements.

Work Search Waiver

The work search waiver enabled SEED participants to pursue their business startup plans full-time rather than actively searching for employment, as is normally required for UI recipients. The duration of the work search waiver was initially set at 10 weeks. Several weeks before the waiver was to expire, treatment group members who had not met all milestones were sent a letter instructing them to contact their BDS for an End of Waiver Period Review to assess their progress in achieving the milestones and to determine whether the work search waiver would be extended. There were three possible results of the review: extend the waiver; not extend the waiver (and return the claimant to the UI system to search for regular employment); or determine that the claimant had met all of the milestones and approve the lump-sum payment.

The most striking feature of the End of Waiver Period Reviews was the limited extent to which such reviews were conducted at all. This was primarily due to the relatively short time between program entry and receipt of the lump-sum payment (see next section). Overall, only 93 claimants (i.e., 12 percent of the treatment group) were recorded as having at least one End of Waiver Period Review. Among those for whom a review was conducted, 75 percent occurred between 9 and 12 weeks after random assignment as planned, and a large majority (77 percent)

resulted in the waiver being extended, with very few claimants (4 percent) being instructed to return to regular UI.

Lump-Sum Payment

To receive a lump-sum payment, SEED participants had to achieve five milestones:

- Completion of the training sessions;
- Development of an acceptable business plan;
- Establishment of a business bank account;
- Satisfaction of all licensing requirements; and,
- Obtaining adequate financing.

In Table 3.4, we provide summary information on the receipt of lump-sum payments in the demonstration. During the demonstration, a total of 451 treatment group members (60 percent) completed all milestones and received a lump-sum payment.[19] Approximately $1.9 million was paid in the form of lump-sum payments, with an average payment of $4,225.

As shown in Table 3.4, the amount of the lump-sum varied considerably among SEED participants. For example, the minimum lump-sum payment was $561 and the maximum was $7,380. One quarter of the recipients received a lump-sum payment of less than $3,077, while another quarter received more than $5,451. The large differences in the lump-sum payment reflect differences in the maximum benefits payable and in the weekly benefit amount, as well as differences in the time required to meet the five program milestones and start a business.[20]

Table 3.4 also provides information on the length of time after random assignment it took for individuals to receive their lump-sum payments. As this table indicates, 44 percent of all treatment group members who received their lump-sum payment received it within 6 weeks of random assignment (or within approximately 4.5 weeks after the business training modules). Another 18 percent took more than 12 weeks after random assignment to complete all of the milestones required to receive the lump-sum payment. Overall, the average length of time after

[19] The proportion of treatment group members who received a lump-sum payment ranged from a low of 47 percent in Wenatchee to a high of 65 percent in Snohomish County.

[20] The average lump-sum payment also varied considerably across site and tended to be much higher in the urban areas of King and Snohomish County and lower in the rural sites. This difference reflects the higher UI entitlement amounts in the urban sites, as well as differences across sites in the time required to meet the five milestones.

Table 3.4
Washington
SEED Lump-Sum Payments

Lump-Sum Payments	All Lump-Sum Recipients (N = 451)
Amount of Lump-Sum Payment ($)	
Minimum	$561
Lowest Quartile	$3,077
Median	$4,360
Third Quartile	$5,451
Maximum	$7,380
Average	$4,225
Time from Random Assignment to Lump-Sum Payment (percent)	
Less than 3 Weeks	9%
3 - 6 Weeks	35%
6 - 9 Weeks	22%
9 - 12 Weeks	17%
12 - 15 Weeks	12%
More than 15 Weeks	6%
Average Number of Weeks to Lump-Sum Payment	7.8

random assignment until receipt of the lump-sum payment was 7.8 weeks. Since it took approximately 4 weeks on average from the effective date of claim to random assignment, this indicates that treatment group members who received a lump-sum payment did so on average within about 12 weeks after their effective date of claim.[21]

[21] There were some site differences in the time taken to achieve the lump-sum payment. For example, treatment group members who received a lump-sum payment in Vancouver, King County, or Wenatchee averaged nearly 9 weeks from random assignment, as compared to roughly 6 weeks in the other three sites. This suggests potential differences across sites in the types of businesses established, the needs of claimants, or the ways in which BDSs assessed the achievement of milestones. Early in the demonstration, we identified a large proportion of SEED participants in Snohomish County receiving their lump-sum payment extremely early. This occurred because the BDS in that site initially treated the milestone review process as pro forma and approved many claimants for their lump-sum payment at the end of the business training modules. This situation was identified during site visits and corrected at an early follow-up training session.

4

MASSACHUSETTS DEMONSTRATION IMPLEMENTATION

In this chapter we provide an overview of the Massachusetts Enterprise Project implementation experiences. As we did earlier for the Washington demonstration, we first describe the flow of claimants through the intake process — from the identification of targeted claimants through random assignment. The effect of the targeting criteria and self-selection is then examined by comparing the demographic characteristics of the UI population, the targeted population, and the treatment group.

Following the assessment of sample characteristics, we provide a detailed description of the changes in demonstration sites and program operations that took place over time. Following this description of implementation changes, we present evidence of the effect of these changes on participation in program activities and on the timing of program activities.

We conclude the chapter by describing the changes in UI regulations that were initiated during the demonstration implementation period. These exogenous changes in the demonstration environment clearly indicate a strong potential for influencing the impact analysis. For this reason we discuss the potential effects of these changes on the analysis results. We also discuss whether, in light of these effects, it is appropriate to pool the three cohorts for the impact analysis.

DEMONSTRATION INTAKE

The Massachusetts Enterprise Project was implemented in three phases. The first phase began demonstration intake in May 1990 and continued for approximately five months; the second intake phase began in April 1991, lasting seven months until October 1991; and the third, and final, intake phase began in March 1992 and continued for approximately one year, until

April 1993. Business support services remained available to demonstration participants for five months after the end of each intake period.

As discussed in Chapter 2, a two-step process was used to identify the target population of new UI claimants invited to participate in the demonstration. The first step was to apply targeting criteria to all new UI claimants and eliminate those who

- were under age 18;
- filed interstate claims; and,
- were eligible for fewer than 26 weeks of UI benefits.

These characteristics were identified using information contained in the UI claims record. The State computer system identified those new claimants in the categories described above and eliminated them from the pool of potential project participants. The claims records for the remaining eligible claimants were then transferred from the State UI mainframe to the demonstration database, the Participant Tracking System (PTS). (This system is described in detail in Chapter 6).

Since the authorizing legislation required that the program focus on unemployed workers likely to exhaust their UI benefits, the second step in identifying the target population was to select those individuals likely to exhaust their UI benefits. To do so, we developed an algorithm to predict each claimant's likelihood of UI benefit exhaustion.[22] The algorithm predicted the probability of UI benefit exhaustion on the basis of information available on the UI claims record, including:

- Total number of dependents;
- Ratio of benefit amount to average wage;
- Whether the claimant was permanently separated from the pre-layoff job;
- Office in which the claim was filed;
- Education level (four categories -- less than ninth grade, some high school, college, and postgraduate education);
- Industry of most recent job (construction, manufacturing, and nondurable manufacturing); and,

[22] For a detailed description of the algorithm, see Benus, et al. "Massachusetts UI Self-Employment Demonstration Interim Report to Congress" in *Self-Employment Programs for Unemployed Workers*, U.S. Department of Labor, Employment and Training Administration, 1992.

- Occupation (professional or clerical).

A numerical probability of exhaustion was calculated for each new claimant by applying the algorithm to the characteristics of each individual record downloaded from the State mainframe. Claimants with a low predicted probability of exhausting UI benefits (i.e., those with a calculated probability below .25) were excluded from the target population for the demonstration. Those in the target population were then sent an invitation to attend the Information Session.[23]

Exhibit 4.1 shows the distribution of the probability of benefit exhaustion (by cohort) for all sample members whose exhaustion probability was .25 or higher. Based on administrative data, we estimate that applying the .25 threshold resulted in eliminating 12 percent of all new UI claimants in Massachusetts who satisfied the initial targeting criteria. Among those included in our sample, the median exhaustion probability was approximately .55 and remained consistent across all three cohorts.

In Table 4.1, we present the affect of applying the above targeting criteria in 1990. Specifically, we compare the characteristics of all UI claimants in the demonstration sites with those who were invited to attend the Information Sessions (i.e., the target group). As indicated by this comparison, new claimants invited to attend the Information Session were similar to the population of all UI claimants in the demonstration sites on most characteristics. Both groups, for example, were similar in terms of age and race and showed only slight differences in prior occupation and industry. In terms of gender, however, larger differences were found between invitees and UI claimants: 60 percent of all UI claimants were male, while only 51 percent of those invited (the target group) were male.

Table 4.1 also provides information for comparing the characteristics of the target group (i.e., those invited to the Information Sessions) with the characteristics of the treatment group. This comparison allows us to examine the effects of self-selection into the demonstration. That is, since only those UI claimants who were interested in self-employment attended the Information Session, and since only the most interested attendees subsequently submitted an

[23] In the two largest sites, Lowell and Woburn, further subsampling was necessary to ensure an even flow of participants from all sites. During 1990 and for part of the 1991 cohort, only 75 percent of all likely exhaustees were randomly selected to receive an invitation. This subsampling was discontinued in July, 1991.

Exhibit 4.1
Probability of UI Benefit Exhaustion by Cohort
Massachusetts Demonstration

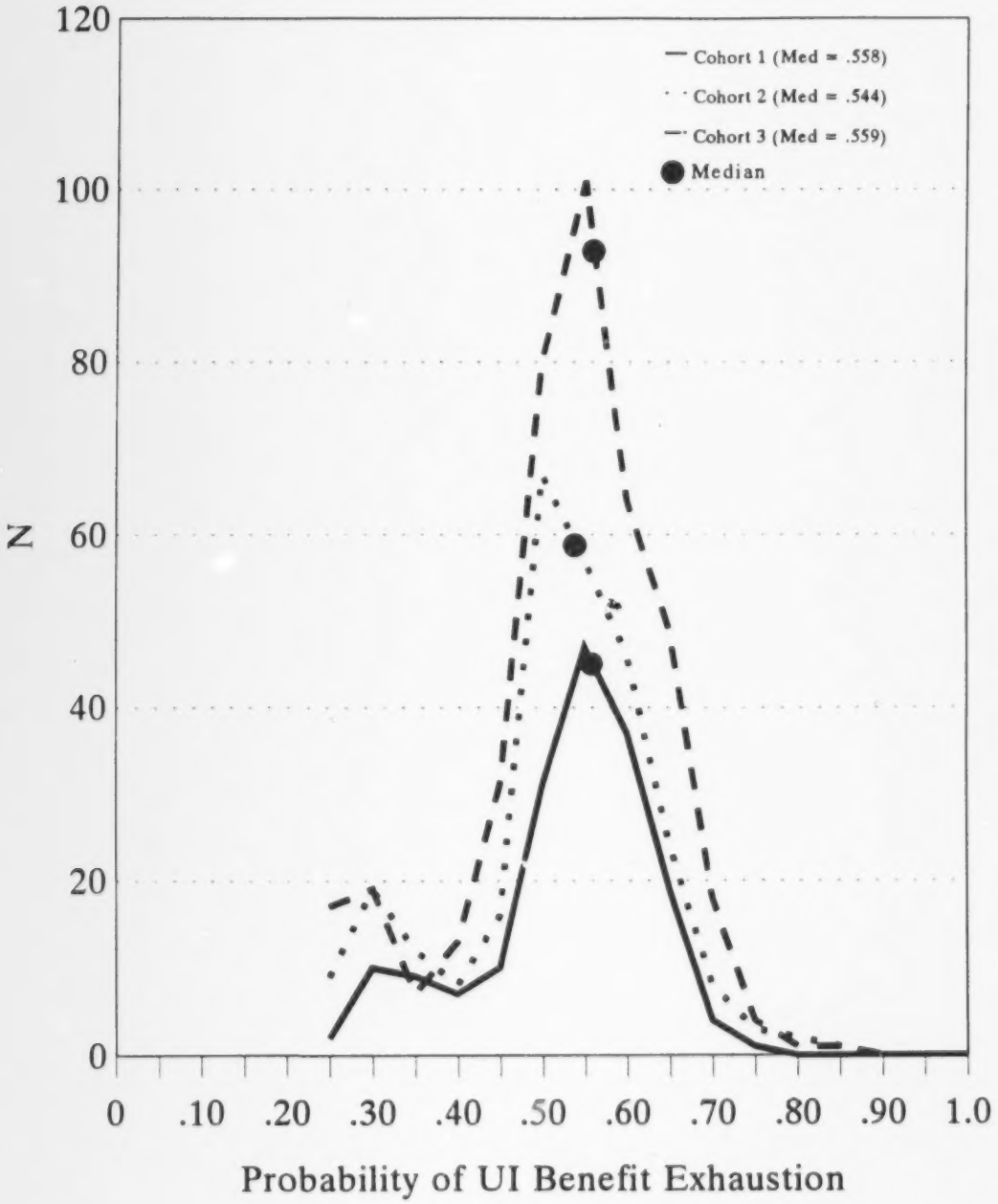


Table 4.1
Massachusetts
Characteristics of UI Claimants,
Targeted Claimants and the Treatment Group
Cohort 1

Characteristics	All UI Claimants Demonstration Sites (N=16,444)	Targeted Claimants (N=10,552)	Treatment Group (N=105)
Gender			
Male	60%	51%	65%
Female	40%	49%	35%
Age at Random Assignment			
less than 25	15%	16%	3%
25-54	73%	73%	93%
55 or older	12%	11%	5%
Mean Age	NA	37	39
Race/Ethnicity			
Caucasian	84%	81%	87%
African American	10%	12%	11%
Hispanic	5%	5%	1%
Other	2%	1%	1%
Education			
High School Graduate	NA	66%	54%
Some College	NA	16%	40%
Mean Education (in years)	NA	13	14
Former Occupation			
Professional/Technical	22%	27%	62%
Clerical	17%	22%	10%
Former Industry			
Manufacturing	26%	30%	24%
Services	22%	25%	37%
Demonstration Site			
Gloucester	6.2%	7.8%	6.6%
Greenfield	4.6%	6.4%	9.5%
Lowell	24.4%	22.3%	22.8%
New Bedford	18.9%	15.4%	8.6%
Roxbury	8.3%	10.0%	6.6%
Springfield	18.4%	21.2%	20.9%
Woburn	19.2%	16.8%	24.7%

Source: PTS and DET's Statewide Summary of Claimant and Jobs Data: September, 1990

application form, the treatment group is composed of individuals who, through their initiative and persistence, selected themselves into the group eligible for random assignment into the program.

As seen in Table 4.1, the target group is nearly evenly divided between males (51 percent) and females (49 percent); the treatment group, on the other hand, is nearly two-thirds male (65 percent). On average, the treatment group is slightly older than the target group (39 years old versus 37 years old). An examination of the age distribution of the two groups reveals that this age difference is largely due to the relative absence of young people from the treatment group (only 3 percent of the treatment group was less than 25 years old as compared with 16 percent for the target group). The treatment group is also more educated than the target group, with a higher average number of years of schooling completed (14 years versus 13 years) and a substantially higher percentage of individuals who attended at least some college (40 percent versus 16 percent). Treatment group members are also more likely than target group members to have previously worked in professional/technical occupations (62 percent versus 27 percent) and more likely to have previously worked in the services industry (37 percent compared with 25 percent). Finally, treatment group members are less likely than the target group to be employed in clerical occupations and in manufacturing industries. In summary, the self-selection of individuals into the demonstration resulted in creating a participant group that may be viewed as "more advantaged" than the target population: slightly older, more educated, and more likely to be in professional occupations.

Tables 4.2 and 4.3 present similar data for UI claimants, target group members, and treatment group members in Cohorts 2 and 3. An analysis of these data reinforce the above findings. Thus, targeting resulted in a group of eligible claimants who were similar in many respects to the general UI claimant population. Those who self-selected themselves into the demonstration, on the other hand, differed from the general UI population on a number of dimensions and yielded a participant group that was more advantaged than the broader target group.

Table 4.2
Massachusetts
Characteristics of UI Claimants,
Targeted Claimants and the Treatment Group
Cohort 2

Characteristics	All UI Claimants Demonstration Sites (N=21,011)	Targeted Claimants (N=15,618)	Treatment Group (N=158)
Gender			
Male	58%	51%	70%
Female	42%	49%	30%
Age at Random Assignment			
less than 25	14%	15%	2%
25-54	75%	74%	88%
55 or older	12%	12%	10%
Mean Age	NA	38	41
Race/Ethnicity			
Caucasian	78%	80%	91%
African American	11%	13%	8%
Hispanic	5%	6%	0%
Other	.8%	1%	1%
Education			
High School Graduate	NA	66%	58%
Some College	NA	18%	39%
Mean Education (in years)	NA	13	14
Former Occupation			
Professional/Technical	23%	27%	49%
Clerical	17%	18%	11%
Former Industry			
Manufacturing	24%	24%	29%
Services	19%	25%	20%
Demonstration Site			
Greenfield	5.0%	5.7%	6.3%
Lowell	11.3%	12.3%	20.8%
Milford	24.9%	21.4%	20.2%
New Bedford	14.1%	10.7%	7.6%
Roxbury	10.2%	11.6%	5.7%
Springfield	17.0%	20.7%	17.7%
Woburn	17.6%	17.5%	21.5%

Source: PTS ad DET's Statewide Summary of Claimant and Jobs Data: September, 1991

Table 4.3
Massachusetts
Characteristics of UI Claimants,
Targeted Claimants and the Treatment Group
Cohort 3

Characteristics	All UI Claimants Demonstration Sites (N= 13,126)	Targeted Claimants (N=37,751)	Treatment Group (N=351)
Gender			
Male	60%	57%	72%
Female	40%	43%	28%
Age at Random Assignment			
less than 25	10%	13%	2%
25-54	77%	75%	90%
55 or older	13%	12%	8%
Mean Age	NA	38	41
Race/Ethnicity			
Caucasian	80%	80%	89%
African American	12%	13%	8%
Hispanic	5%	6%	2%
Other	2%	2%	1%
Education			
High School Graduate	NA	69%	52%
Some College	NA	15%	45%
Mean Education (in years)	NA	13	14
Former Occupation			
Professional/Technical	28%	26%	60%
Clerical	19%	17%	7%
Former Industry			
Manufacturing	27%	26%	22%
Services	24%	25%	30%
Demonstration Site			
Greenfield	5.8%	5.2%	5.9%
Lowell	25.9%	24.4%	17.9%
Milford	11.7%	11.0%	18.2%
Northampton	6.7%	7.5%	10.8%
Roxbury	11.6%	12.2%	7.9%
Springfield	20.3%	20.3%	14.2%
Woburn	18.0%	19.4%	24.8%

Source: PTS and DET's Statewide Summary of Claimant and Jobs Data: May, 1993

PARTICIPANT CHARACTERISTICS

In the previous section, we analyzed the effect of targeting and self-selection on the demonstration sample. We concluded that targeting and self-selection had a consistent effect in each of the three cohorts. In this section, we examine the characteristics of the resulting samples.

The demographic and other characteristics of sample members in each of the three cohorts are presented in Table 4.4.[24] As indicated in this table, Cohorts 1, 2, and 3 are quite similar: approximately two-thirds are male; the mean age is approximately 41 years old; the vast majority is Caucasian; and the average number of years of education is 14. We conducted a t-test of differences in means or proportions for each of the characteristics in the table and found none of the cohort differences to be statistically significant at the .05 level. Based on this analysis, we conclude that Cohorts 1, 2, and 3 are quite similar.

As discussed in Chapter 3, a critical evaluation issue concerns the comparability of the individuals in the treatment and control groups. We compared the characteristics of the treatment and control groups (results not presented here) and found that the random assignment process was very successful in generating two groups that were remarkably similar on all of the standard characteristics collected at the time the claim was filed. In addition, the groups are extremely similar on prior earnings and prior business experience. We conducted t-tests of differences in means on these characteristics and found that none of the differences was statistically significant at the .05 level. Thus, we conclude that differences in outcomes observed for treatment and control group members can be confidently attributed to the demonstration.

CHANGES IN PROGRAM IMPLEMENTATION

As described above, the Massachusetts Enterprise Project was implemented in three distinct phases. During the first enrollment period, which operated from May through September 1990, 207 individuals were randomly assigned to treatment or control status. This

[24] For this analysis we combine treatment and control group members.

Table 4.4
Massachusetts
Characteristics of Experimental Sample
(Treatments and Controls)
Cohorts 1, 2, and 3

Characteristics*	Cohort 1 (N=207)	Cohort 2 (N=314)	Cohort 3 (N=701)
Gender			
Male	67%	67%	70%
Female	33%	33%	30%
Age at Random Assignment			
Mean Age (in years)	39.8	41.1	40.7
Race/Ethnicity			
Caucasian	89%	90%	89%
African American	9%	8%	9%
Hispanic	1%	1%	2%
Other	1%	1%	1%
Education			
Percent College Graduate	43%	45%	45%
Mean Education (in years)	14.3	14.6	14.5
UI Entitlement ($)			
Mean Weekly Benefit Amount	$244	$250	$268
Mean Maximum Benefit Payable	$7,249	$7,448	$7,990

Source: PTS

All values shown in the table are based on non-missing values. Therefore, the sample size for different variables may vary slightly.

enrollment period is referred to as Cohort 1 (or the 1990 sample). The Cohort 2 (or the 1991 sample) enrollment period lasted from April through October of 1991, during which time 314 claimants were enrolled in the experimental sample. Cohort 3 (or the 1992-93 sample) enrollment lasted from March 1992 through April 1993, assigning a total of 701 individuals to

treatment and control status. Thus the Massachusetts demonstration includes 1,222 UI claimants.

While the overall project design did not change between program years, some program features did change during the three-year implementation period. Since these changes may influence and/or help explain the analytic results presented in subsequent chapters, we highlight these changes below.

Changes in Demonstration Sites

Perhaps the most important change in the Enterprise Project implementation was the change in demonstration sites. In 1991, for example, Milford was added and Gloucester was dropped from the demonstration. The primary reason for this change was to increase the flow of claimants into the demonstration (Milford is substantially larger than Gloucester). The remaining sites (Greenfield, Lowell, New Bedford, Roxbury, Springfield, and Woburn) operated the demonstration in both 1990 and 1991.

For the final phase of the demonstration (1992-93), an additional change in sites took place: New Bedford was dropped and Northampton was added. This change was necessitated by the fact that New Bedford was selected as a site for the Massachusetts Industrial Services Program (ISP), another entrepreneurial training program. The selection of New Bedford as an ISP site increased the likelihood that Enterprise Project control group members would have received entrepreneurial training services through the ISP program, thus, potentially confounding the demonstration results. Furthermore, the Massachusetts Department of Employment and Training (DET) commissioner determined that it would be inappropriate to operate two entrepreneurial training programs in one city while many other cities had no entrepreneurial program at all. As a result, DET recommended that we drop New Bedford as a site and add Northampton.[25] Thus, only five sites (Greenfield, Lowell, Roxbury, Springfield, and Woburn) operated the program in all three phases. Milford and New Bedford operated the program for

[25] This change had another advantage. Northampton could combine with Greenfield for the biweekly Enterprise Workshops, reducing the travel time for Greenfield participants (who previously had to travel to Springfield for many sessions).

two of the three phases and Gloucester and Northampton for one year. These site changes and the location of the demonstration sites are depicted in Exhibit 4.2.

Changes in Program Operations

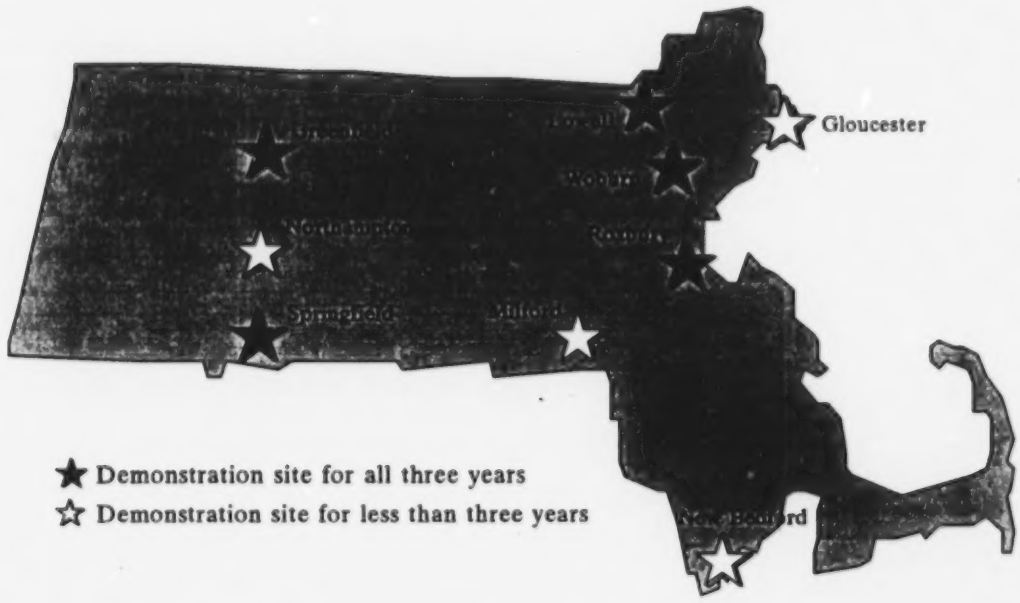
Other significant changes occurred in the delivery of business assistance services. Initially, business support services were provided by the Massachusetts Small Business Development Centers (MSBDC) and the Franklin County Community Development Corporation. Based on feedback from demonstration participants and DET's evaluation of program services during the first year, DET determined that a change in service providers was needed to improve the responsiveness of business support services to the needs of the clients. Prior to the second implementation phase, therefore, DET contracted with four business development organizations to provide business support services for Cohort 2 members. Three of the four were retained for Cohort 3 and a new provider was added to better serve the Roxbury site.

While efforts were made to maintain consistency in training services during all three phases, some changes were necessary to meet client needs and to improve service delivery. For example, the length of the Enterprise Seminar (the first self-employment training session) was reduced from eight hours in 1990 to 4.5 hours in 1991. This change was precipitated by feedback from participants and business development specialists who indicated that an eight-hour session resulted in fatigue for participants and made it difficult for them to absorb all of the information presented. To accommodate the shorter timeframe for the Enterprise Seminar, the curriculum was altered to eliminate the detailed discussion of financial and personnel issues that had been included in the Cohort 1 Enterprise Seminars.[26] The eliminated topics were added to the bi-weekly session curricula.

For the 1992-93 phase, the Enterprise Seminar was lengthened slightly to 5.5 hours. This change was based on recommendations from the business development specialists who found that 4.5 hours was not quite sufficient time for presenting the material. While the

[26] Additionally, the 1990 Enterprise Seminar included an introductory discussion of the Massachusetts Small Business Development Centers and their role in the Enterprise Project. This material was not relevant for the 1991 sessions and was therefore eliminated.

Exhibit 4.2
Massachusetts Enterprise Project Demonstration Sites During the Three Implementation Phases



Site	1990	1991	1992
Gloucester	*		
Greenfield	*	*	*
Lowell	*	*	*
Milford		*	*
New Bedford	*	*	
Northampton			*
Roxbury	*	*	*
Springfield	*	*	*
Woburn	*	*	*

Source: Massachusetts DET

BEST COPY AVAILABLE

curriculum was not altered, slightly more time was allocated for each topic and a lunch break was added.

The curricula and schedules for the bi-weekly sessions[27] were significantly altered following the first year of implementation in order to increase consistency across the demonstration sites. For Cohorts 2 and 3, standard curricula were developed for each of the six workshops: (1) Cash Flow and Financial Statements; (2) Personal Effectiveness and Selling; (3) Financing; (4) Marketing; (5) Legal Issues/Insurance; and, (6) Bookkeeping/Taxes.

For Cohort 2, the schedule was organized so that participants could attend all six workshops over a 12-week period. Beginning in Cohort 3, the schedule for the workshop sessions was shortened so that all six sessions could be completed within nine weeks (rather than twelve). This change was made to accelerate participants' progress through the demonstration activities and to assist participants in starting their businesses more quickly. In addition to modifying the schedule for the workshops, DET developed the schedule so that the three workshops deemed most critical to early business development -- Cash Flow and Financial Statements, Personal Effectiveness and Selling, and Marketing -- were scheduled to occur first. DET also established a staggered schedule of workshops throughout the State so that participants could quickly make up missed workshops in other locations. Furthermore, each bi-weekly workshop was increased from 2 to 2.5 hours to allow more time to cover each topic.

Cohort 3 participants were required to present their business plans to their fellow participants during the workshop sessions. This change was made at the suggestion of the business development counselors, who felt that requiring an oral presentation would assist participants in developing their marketing skills as well as self-assess their progress.

Another change, instituted for Cohort 3, was the addition of a mid-program review. In this mid-program review, the business development counselor and the participant met to evaluate the participant's progress toward starting the business. As part of this review, the counselor, together with the participant, decided whether or not it was appropriate for the participant to continue in the demonstration. It is important to note that maintaining satisfactory progress was always a requirement of the demonstration. The addition of the mid-program review can thus

[27] The name of these sessions was changed in 1991 to Enterprise Workshops.

be viewed as a mechanism for ensuring that demonstration participants make satisfactory progress towards starting their businesses, although satisfactory progress was always a requirement of the Massachusetts demonstration. It should also be noted that the mid-program review was similar to the End of Waiver Period/Review in the Washington demonstration.

Participation in Enterprise Project Activities

Information on the participation rates in various Enterprise Project activities across the three phases is shown in Table 4.5. As indicated in the table, the percentage of individuals who attended an Information Session remained fairly constant over time: 3.5 percent in Cohort 1, 4.1 percent in Cohort 2, and 4.3 percent in Cohort 3. The application rate (i.e., the percentage of Information Session attendees who submitted an application) dropped markedly over time, from a high of 69 percent in Cohort 1 to 53 percent in Cohort 3. The combination of increasing attendance rates and decreasing application rates yields a relatively constant overall takeup rate (i.e., the rate of participation among those eligible to participate). In Cohort 1, 2.4 percent of all invited claimants attended the Information Session *and* submitted an application. This rate remained at 2.4 percent in Cohort 2 and was 2.3 percent in Cohort 3.

The bottom panel of Table 4.5 provides the rate of participation in business assistance services among those eligible to participate. These results indicate substantial similarity between Cohorts 1 and 2, with more than 92 percent of all treatment group members attending the Enterprise Seminar and approximately 90 percent attending at least one counseling session. Cohort 3 results indicate slightly higher attendance rates, with 94 percent attending the Enterprise Seminar and 94 percent attending at least one individual counseling session.

Table 4.5 also indicates a steady increase in the proportion of treatment group members attending at least one bi-weekly session: 77 percent in Cohort 1, 90 percent in Cohort 2, and 92 percent in Cohort 3. This increase in attendance over time may reflect the more formal organization of the sessions, the standardization of curricula, and the tightening of the schedule for completing all workshops.

Table 4.5
Massachusetts
Participation In Various Enterprise Project Activities

Activity	Cohort 1		Cohort 2		Cohort 3	
	Number	Percent	Number	Percent	Number	Percent
Invited to Information Session	10,552		15,618		37,751	
Attended Information Session*	372	3.5%	641	4.1%	1,645	4.3%
Submitted an application*	257	69% (2.4% of all invited)	381	59% (2.4% of all invited)	877	53% (2.3% of all invited)
Randomly Assigned*	207	81%	314	82%	701	80% (1.8% of all invited)
Treatment Group	105	(1.9% of all invited)	158	(2.0% of all invited)	351	
Control Group	102		156		350	
Business Assistance Services Received by Treatment Group Members						
Enterprise Seminar	97	92%	146	92%	330	94%
At least one counseling session	94	90%	145	92%	330	94%
At least one bi-weekly workshop	81	77%	142	90%	322	92%

Source: PTS

* The percentages shown are percentages of the activity above.

TIMING OF ENTERPRISE PROJECT ACTIVITIES

The interval between key Enterprise Project activities in Cohorts 1, 2, and 3 is presented in Table 4.6. For most activities, the elapsed time between consecutive activities was somewhat shorter for Cohorts 2 and 3 than for Cohort 1. For example, the average elapsed time from application receipt to random assignment decreased from 10 days in Cohort 1 to 8 days in Cohorts 2 and 3. The total number of days between the benefit-year-begin date and attendance at the Enterprise Seminar was 64 days in Cohort 1 and 58 days in Cohorts 2 and 3.

Table 4.6
Massachusetts
Average Number of Days Between Key Program Activities
Cohorts 1, 2, and 3

Activity	Cohort 1	Cohort 2	Cohort 3
	Mean Days	Mean Days	Mean Days
Benefit Year Begin Date to Invitation Date	22	23	23
Invitation Date to Date of Information Session	12	10	10
Information Session Attendance to Application Receipt	6	6	5
Application Receipt to Random Assignment	10	8	8
Random Assignment to Enterprise Seminar	14	11	12
Benefit Year Begin Date to Enterprise Seminar	64	58	58

CHANGES IN UI REGULATIONS

As discussed in Chapter 1, Section 9152 of the Omnibus Budget Reconciliation Act (OBRA) of 1987 authorized the Massachusetts Demonstration. This legislation required the State to reimburse the Unemployment Trust Fund any excess cost resulting from the self-employment demonstration. Excess costs arise when demonstration treatment group members, on average, collect more self-employment allowances than the amount of UI benefits they would have collected in the absence of the demonstration (as measured by the experience of control group

members). Thus, excess costs are measured as the difference in benefits paid to treatment and control group members.

As part of the demonstration design phase, the Massachusetts DET, together with DOL and the research contractor, developed a variety of demonstration features designed to reduce the potential for excess costs while maintaining all the legislative requirements.[28] One important design feature was the statistical algorithm to identify likely UI exhaustees. This algorithm was used throughout the demonstration to offer Enterprise Project services only to UI claimants who exceeded a threshold exhaustion probability (as required by the authorizing legislation). A second important design feature was the establishment of a 24-week work search waiver period for Enterprise Project participants. As a result of this demonstration design feature, treatment group members could collect self-employment allowances only through the 24th consecutive week of their UI claim. At that point, treatment group members had to choose between continuing with their self-employment activities full-time or returning to UI for their remaining six weeks of benefits and meeting the regular work search requirements.

Thus, the design of the Massachusetts Enterprise Project required treatment group members to forego up to 6 weeks of UI benefits if they chose to continue their self-employment activities full-time beyond the 24th week of their UI claim. With an average weekly benefit rate of $244 (in 1990), treatment group members thus faced a tradeoff between continued pursuit of self-employment and UI benefits valued at $1,464. Control group members did not have to make such a choice since they could collect up to the maximum duration of UI benefits -- 30 weeks in Massachusetts.

Starting in November 1991 and continuing through October 1993, a variety of changes dramatically affected the tradeoff for treatment group members. For example, effective in November 1991, the Emergency Unemployment Compensation (EUC) Act of 1991 provided for up to 20 weeks of additional EUC benefits beyond the 30 weeks of UI benefits available to Massachusetts claimants. Subsequent extensions of EUC (there were four additional EUC periods) provided for up to 33 weeks of EUC benefits. Thus, during part of the demonstration

[28] Similar design features were not incorporated into the Washington demonstration since the Washington demonstration was funded through DOL research funds and, thus, not required to reimburse excess costs.

period, treatment group members faced a tradeoff of substantially more than 6 weeks of UI benefits.

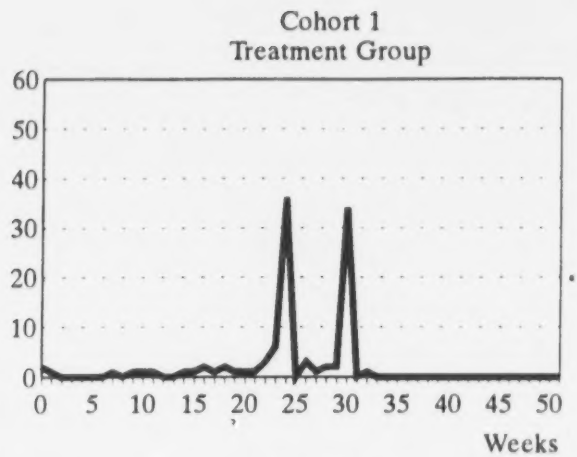
While it is impossible to quantify the exact tradeoff faced by treatment group members throughout the three-year implementation period, we can calculate the initial impact on the tradeoff resulting from the passage of the EUC Act. Treatment group members who were eligible for EUC in November 1991, faced a tradeoff of 26 weeks (6 weeks of UI benefits plus 20 weeks of EUC benefits). With an average weekly benefit rate of $250 (in 1991), the value of the tradeoff was thus raised to $6,500 by the passage of the EUC Act of 1991.

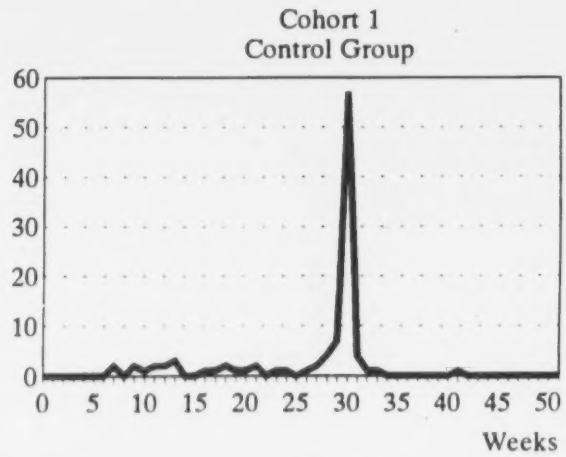
The effect of this dramatic increase in the tradeoff facing treatment group members is impossible to quantify. It is clear, however, that when facing a substantially higher tradeoff, treatment group members are less likely to continue their self-employment activities beyond week 24. Thus, we expect to observe fewer treatment group members in Cohort 3 continuing with their self-employment effort beyond week 24 as compared with treatment group members in Cohorts 1 and 2. Thus, holding other factors constant, smaller program impacts in Cohort 3 are expected as compared with Cohorts 1 and 2.

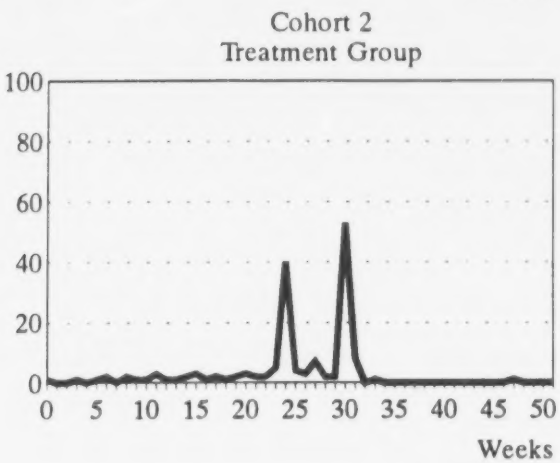
The Massachusetts impact results are also likely to be affected by another change in UI regulations. In 1992, the Massachusetts legislature passed a law limiting UI benefits to 26 weeks (from 30 weeks) during periods when EUC or any other federal extended unemployment benefits program was in effect. The impact of this change in the law on the behavior of program participants is unknown. The change, however, is expected to affect our analysis of UI benefits collected. That is, since the data discussed here include only UI benefits (and exclude other benefits such as EUC), the impact of this change is likely to distort our analysis of UI benefits receipt. We explore this issue with the aid of a graph below.

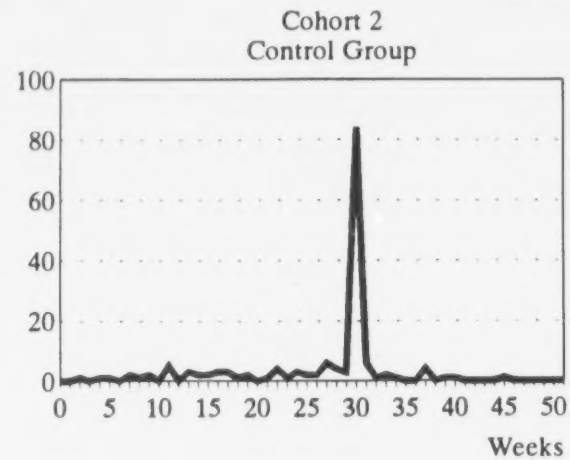
Exhibit 4.3 depicts the number of weeks of UI benefits collected by treatment and control group members in each cohort. As indicated in the top panel of the exhibit, approximately an equal number of treatment group members in Cohort 1 collected either 24 or 30 weeks of UI benefits, while the majority of control group members collected 30 weeks of benefits. This pattern is repeated for members of Cohort 2, where approximately an equal number of treatment

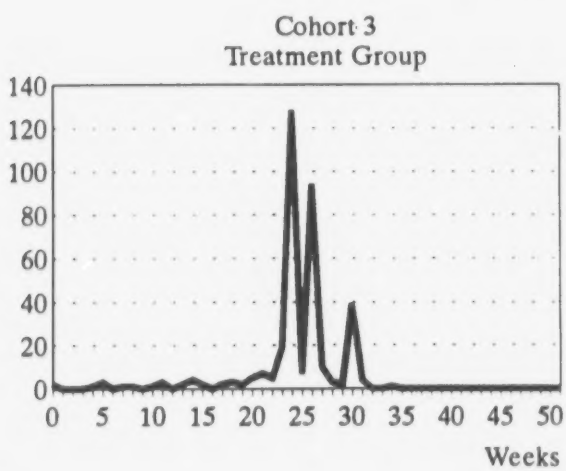
Exhibit 4.3
Weeks of UI Benefits by Treatment and Control
Cohorts 1, 2 and 3
Massachusetts Demonstration











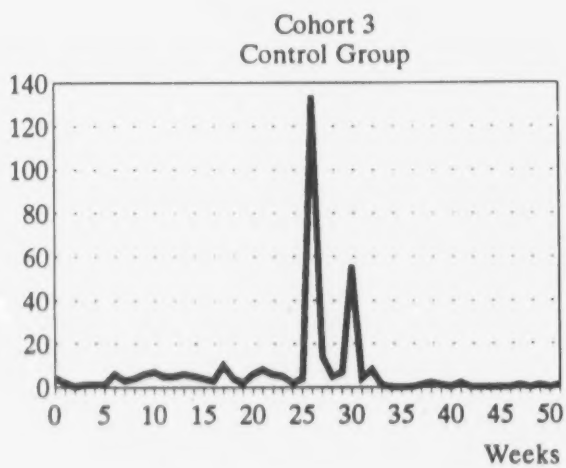
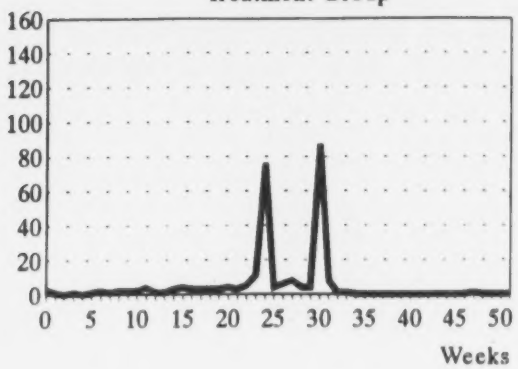
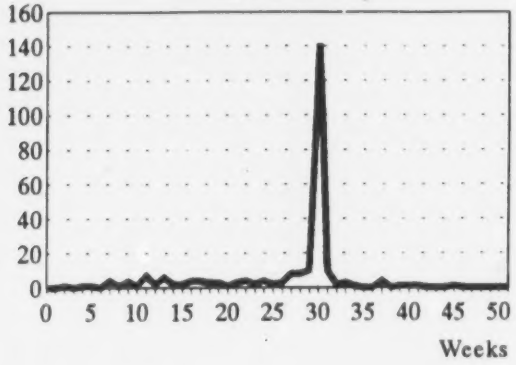


Exhibit 4.4
Weeks of UI Benefits by Treatment and Control
Cohorts 1&2 and Cohort 3
Massachusetts Demonstration

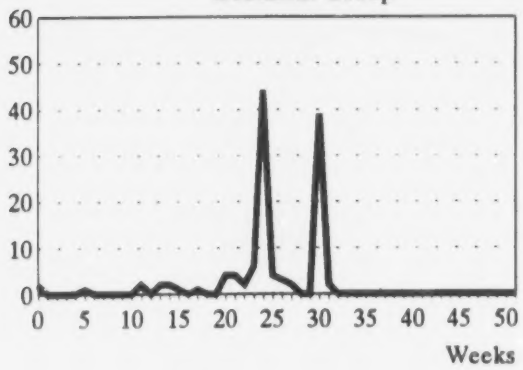
Cohorts 1 & 2
Treatment Group



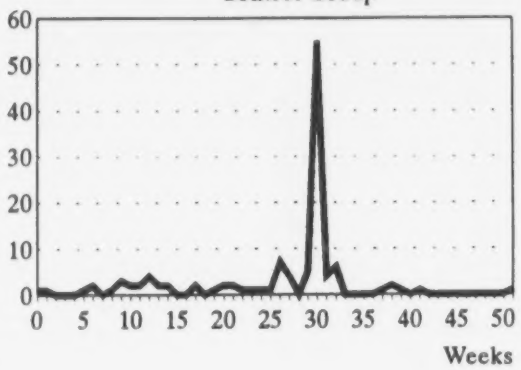
Cohorts 1 & 2
Control Group



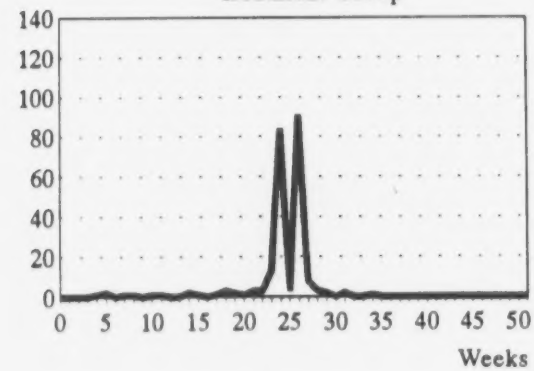
Cohort 3, Enrolled Before June 1, 1992
Treatment Group



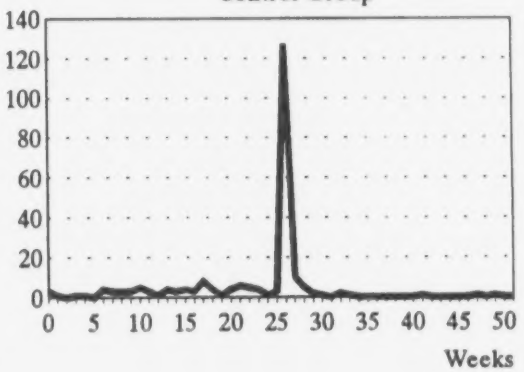
Cohort 3, Enrolled Before June 1, 1992
Control Group



Cohort 3, Enrolled After June 1, 1992
Treatment Group



Cohort 3, Enrolled After June 1, 1992
Control Group



group members also collected either 24 or 30 weeks of benefits and the majority of the control group collected 30 weeks of UI benefits.

The pattern, however, is substantially different for Cohort 3 members. As shown in the bottom panel of Exhibit 4.3, the distribution of weeks of UI benefits has three peaks for the treatment group, and two peaks for the control group. That is, in Cohort 3, the majority of treatment group members collected either 24, 26, or 30 weeks of benefits. That is, 128 collected 24 weeks of UI benefits, 94 collected 26 weeks, and 39 collected 30 weeks. Within the control group, the majority collected either 26 or 30 weeks of benefits (134 and 56, respectively). These results for Cohort 3 suggest that the availability of Federal supplemental benefits, together with the change in the UI law, appears to have altered the behavior of Cohort 3 relative to the earlier cohorts.

To investigate the results of Cohort 3 further, we split the sample into two groups: those who filed a UI claim before June 1, 1992 (i.e., the effective date of the State UI law change) and those who filed after that date. In Exhibit 4.4, we present the distribution of number of UI weeks collected by the combined Cohort 1 and Cohort 2 sample (top row). In the second row of the exhibit, we present the results for Cohort 3 members who filed before June 1, 1992; the third row shows results for those who filed after that date. An examination of this exhibit reveals that those who filed before June 1 have a pattern similar to the combined Cohorts 1 and 2 sample; that is, approximately an equal number of treatment group members collected either 24 or 30 weeks of UI benefits and a majority of the control group collected 30 weeks of benefits.

Those in Cohort 3 who filed after June 1 (bottom row), showed an entirely different pattern. Within the treatment group, approximately an equal number collected either 24 or 26 weeks of UI benefits. Within the Control group, a majority collected 26 weeks of UI benefits. This decomposition of Cohort 3 suggests that it is composed of two distinct subgroups -- those who filed an initial UI claim before the effective date of the law change and those who filed after that date. Cohort 3 thus operated under two very different environments.

Based on the above analysis of UI benefits receipt by cohort, can we combine all three cohorts for the impact analysis? Alternatively, should we combine part of Cohort 3 (i.e., those who filed before the change in the law) with Cohorts 1 and 2? We take the conservative approach of estimating impacts separately for Cohort 3, while combining Cohorts 1 and 2.

5

COMPARISON OF DEMONSTRATION IMPLEMENTATION

In the previous two chapters, we presented the implementation experiences of the Washington SEED Demonstration and the Massachusetts Enterprise Project separately. In this chapter, we compare the implementation experiences of the two demonstrations, in an attempt to gain insights that will help explain differences in program impacts.

In this chapter, we compare the experience of the entire SEED Demonstration with the entire Enterprise Project Demonstration. For this analysis, therefore, we combine all three Massachusetts cohorts and compare their combined implementation results with the results of the Washington sample. In the sections below, we first compare the timing of the two demonstration intake periods. Next, we compare the participation rates in various demonstration activities (e.g., attendance at orientation, application, random assignment, receipt of business services). Then, we compare experiences of demonstration participants, their assessment of demonstration services, and throughout this chapter, we place special emphasis on identifying implementation differences that may lead to program impact differences.

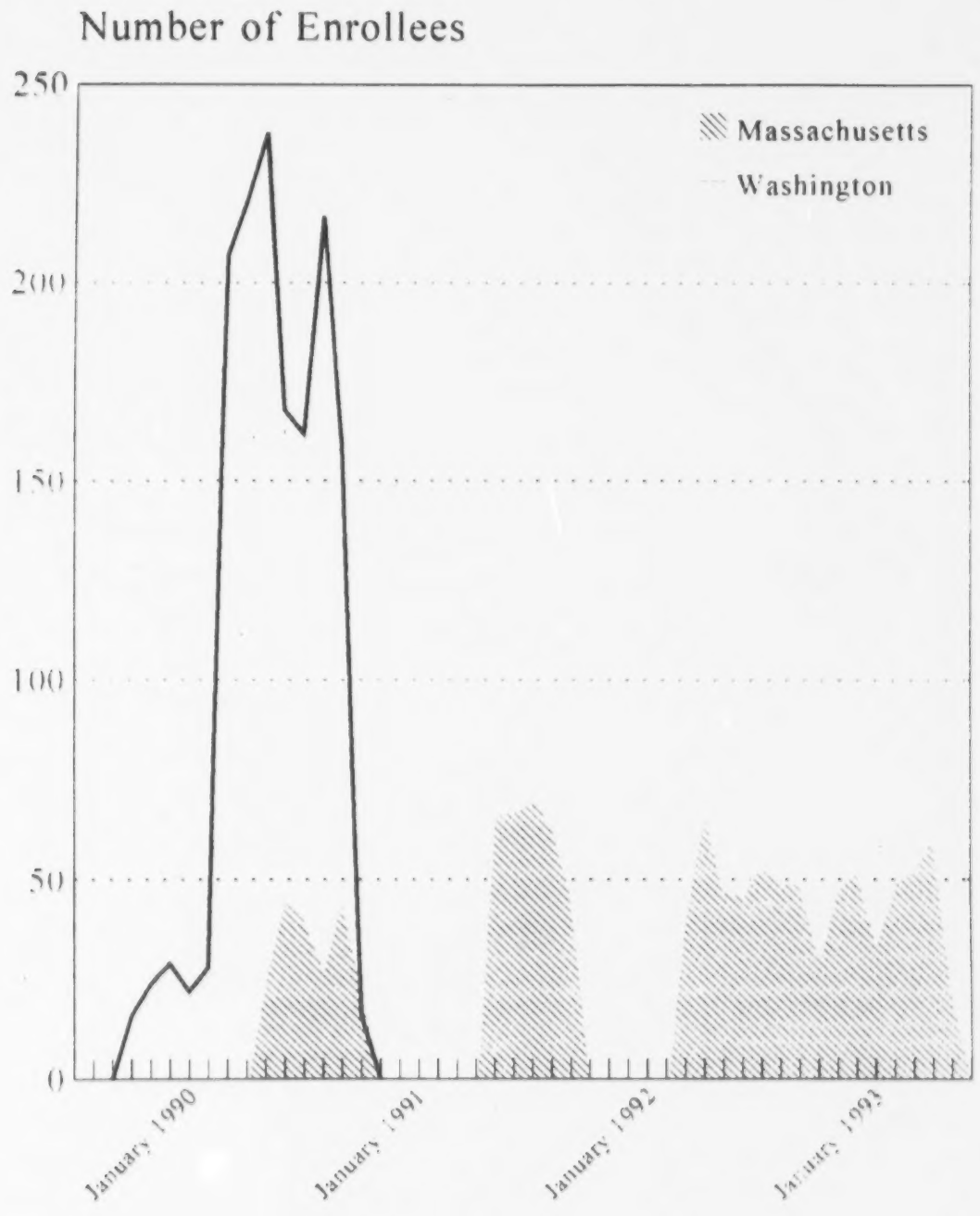
Timing of the Intake Periods

Perhaps the most important implementation difference between the two demonstrations is the difference in intake periods. In Washington, there was a single intake period, from September 1989 to September 1990. In contrast, the Massachusetts demonstration had three distinct intake periods. The first intake period started in May 1990 and the last concluded in April 1993. Both the Washington and Massachusetts intake periods are depicted in Exhibit 5.1.

During the single Washington intake period, a total of 1,507 Washington SEED applicants were randomly assigned to either the treatment group (755) or the control group (752). During the three distinct Massachusetts intake periods, a total of 1,222 Massachusetts

Exhibit 5.1

Demonstration Enrollment Over Time



Enterprise Project applicants were similarly assigned to the either the treatment (614) or the control (608) group. Thus, in terms of sample size, the Washington demonstration was larger than the Massachusetts demonstration (1,507 versus 1,222).

As is clear from Exhibit 5.1, the three Massachusetts intake periods lasted substantially longer than the Washington intake period. From the beginning of the first intake period (in May 1990) to the end of the last intake period (in April 1993) nearly three years had passed. Even if we exclude the gaps between the intake periods, the actual intake periods in Massachusetts lasted a total of approximately two years as compared with one year for the Washington demonstration.

Table 5.1
Washington/Massachusetts Comparison
Participant Flow

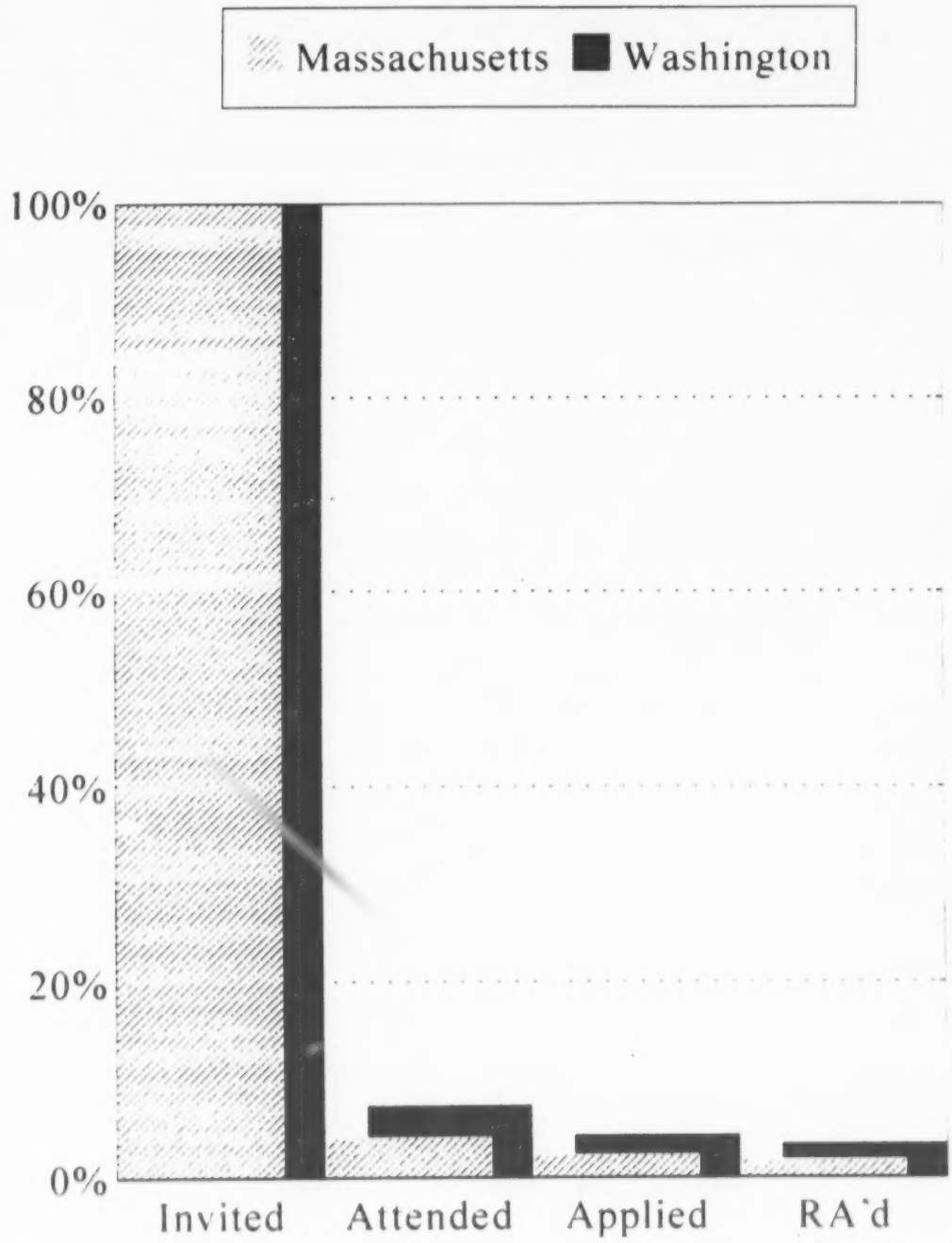
Activity	Massachusetts		Washington	
	Number	Percent	Number	Percent
Invited to Information Session	63,921		42,350	
Attended Information Session	2,658	4.2%	3,167	7.5%
Submitted an application	1,515	57.0%	1,932	61.0%
		(2.4% of all invited)		(4.5% of all invited)
Randomly Assigned	1,222	80.7%	1,507	78.0%
Treatment Group	614	(1.9% of all	755	(3.6% of all
Control Group	608	invited)	752	invited)

Source: Participant Tracking System

ATTENDANCE AT ORIENTATION

In Table 5.1, we present a comparison of the participant flow through various stages of the demonstrations. This flow is also depicted in Exhibit 5.2. An examination of Exhibit 5.2 indicates substantial similarity in the two flow patterns. That is, only a small portion of the target groups in both demonstrations chose to attend an information session. This pattern

Exhibit 5.2
Participation in Various Project Activities
Massachusetts and Washington



reinforces the observation that self-employment is not for everyone. In fact, most UI claimants who are offered an opportunity to participate in a self-employment program, choose not to attend a meeting where they can get additional information about the self-employment program. Among those who expressed an interest (or, at least, a curiosity) by attending the orientation meeting, approximately three-fifths submitted an application and approximately four-fifths were accepted and randomly assigned in both demonstrations.

As indicated in Table 5.1, in Washington, a total of 42,350 UI claimants were invited to attend a SEED orientation session; in Massachusetts, a total of 63,921 UI claimants were invited to attend an Enterprise Project orientation session (combining the three years of the Massachusetts demonstration). The response rate to the invitations was substantially higher in Washington than in Massachusetts. In Washington, 7.5 percent of those invited to attend an orientation session were interested enough in the possibility of pursuing self-employment that they chose to attend the session. In Massachusetts, 4.2 percent of those invited to an orientation session attended. The attendance rate in Washington is thus nearly twice the attendance rate in Massachusetts.

There are several possible explanations for the difference in attendance rates. First, the contents of the two invitation letters were different and may have influenced the attendance rate. Second, claimants may have been invited to the orientation at different stages of their unemployment spell. Third, the target populations in the two demonstrations may have differed on some characteristic that influences interest in self-employment. Fourth, knowledge of the lump-sum payment (in Washington) may have encouraged more people to participate. Finally, there may have been other factors, such as local economic conditions that affected the attendance rates. We examine each of these factors below.

The content and wording of the invitation letters may have affected the attendance rates. In Exhibits 5.3 and 5.4 we present the invitation letters used in the two demonstrations. In the Washington letter (Exhibit 5.3), the first sentence asks simply "Are you interested in self-employment?" In the Massachusetts letter (Exhibit 5.4), the first sentence asks a somewhat more narrow question "Do you have an idea for your own business?" Thus, in Washington, individuals with a general interest in self-employment but with no specific business idea were invited to attend. In Massachusetts, on the other hand, the wording of the letter targeted the invitation to only those individuals with a business idea. This may partly explain why relatively fewer individuals in Massachusetts attended the orientation session.

Exhibit 5.3
Washington
Invitation Letter

Name
Address 1
Address 2
City, State
Zip

SSA Letter
JSC
BYE
Current Date

Are you interested in self-employment? If so, you are invited to attend a one-hour self-employment Awareness Day meeting:

Date _____
Location _____
_____
_____
_____

(Please arrive early, latecomers will not be allowed)

The State of Washington is conducting a special project for people interested in self-employment. The regular unemployment insurance system does not allow claimants to receive benefits and pursue full-time self-employment. But the purpose of the Self-Employment & Enterprise Development (SEED) Project is to find ways to make the Unemployment Insurance system more effective for people who would like to start their own business.

Participants in the SEED project will receive unemployment benefits and training while preparing to start their own business. The project is designed for individuals who already have a business idea and are prepared for a quick start. You must also be fully eligible to receive unemployment benefits to participate.

This is a one-time offer. YOU MAY NOT RESCHEDULE. To learn more about this project and help you decide if you wish to apply, you must personally attend Awareness Day on the date that appears above. Attendance at this meeting will not affect your eligibility for unemployment benefits.

If you have questions about the SEED project, please do not contact your Job Service Center. You may call the SEED Project Unit at 1-800-782-9099 or 1-206-586-8849.

Exhibit 5.4
Massachusetts
Invitation Letter

(Name)
(Date)
(Address)

Dear (Name):

Do you have an idea for your own business? Would you like to try to turn your idea into a full-time job?

The Enterprise Project is a pilot program that the Commonwealth of Massachusetts is offering in seven selected communities for people who are receiving unemployment insurance and want to start their own businesses. To find out about the program and to learn a little about self-employment you should attend one of the following Information Session:

- (Date) (Site)
  (Time) (Address)

OR

- (Date) (Site)
  (Time) (Address)

OR

- (Date) (Site)
  (Time) (Address)

At this meeting, a representative from the Department of Employment and Training will explain how the Enterprise Projects ties in with your unemployment compensation. A business counselor will discuss the risks and rewards of self-employment and help you begin to evaluate your business idea.

To qualify for the Enterprise Project, you must have attended an information session and have a clear business idea which will be reviewed for final eligibility. The Project is only open to people who will not be recalled to their previous job. The Project is very small. Applicants will be selected by lottery. I will be happy to answer any questions you may have about the program at the information session.

Sincerely,

(Enterprise Rep. Name)
Enterprise Representative
(Site) Opportunity Job Center

Moreover, the Massachusetts letter mentions that the Enterprise Project is very small and that applicants will be selected by lottery. The Washington letter, on the other hand, makes no mention of random selection or limited enrollment. The Washington letter may thus be viewed as less restrictive and somewhat more encouraging to invitees than the Massachusetts letter. dT timing of the invitation letter and the scheduling of the orientation sessions may have also influenced the attendance rates in the demonstrations. As described earlier, the time interval between the benefit year begin date and the orientation attendance date was shorter in Washington than in Massachusetts (by approximately 2 weeks).[29] If some Massachusetts claimants found jobs during the extra interval, they would not have attended the orientation session. Given the high average duration of unemployment in Massachusetts, however, it is unlikely that the extra two weeks had a significant impact on the attendance rate.

An examination of the demographic characteristics of the target populations in the two demonstrations (shown for Washington in Table 3.1 and for Massachusetts in Tables 4.1, 4.2, and 4.3) shows that the Massachusetts target group (those sent invitations) are more educated, slightly older, and more likely to have previously worked in professional/technical occupation and in the manufacturing industry as compared to the Washington target group.

There is some anecdotal evidence that individuals in Washington filed claims in demonstration sites in order to be eligible for the demonstration and, more specifically, the lump-sum payment. During the pilot phase, for example, when Vancouver was the only demonstration site, some individuals actually traveled from Seattle to Vancouver (180 miles) to file a claim. To the extent that this occurred, it would contribute to the higher response rate in Washington.

Finally, economic conditions in Washington were relatively robust during the SEED enrollment period. In Massachusetts, on the other hand, economic conditions were very poor throughout the demonstration. Prior evidence suggests that the local unemployment rate is positively related to interest in self-employment (i.e., higher unemployment rates are related to

[29] The reason for this difference is largely a result of the fact that, during the demonstration, Washington was a wage reporting state while Massachusetts was not. This resulted in a longer period of time needed to determine monetary eligibility for UI in Massachusetts. Following the conclusion of the demonstration, Massachusetts has also become a wage reporting state.

increased interest in self-employment). Thus, economic conditions do not help to explain the higher attendance rates in Washington.

APPLICATION

After attending the initial orientation session, the next step in the intake process in both demonstrations was filing an application. At the end of the initial session, those interested in applying to the demonstration took an application packet and were required to submit the completed package within seven days. As indicated in Table 5.1, the application rate among information session attenders in Washington was only slightly higher than in Massachusetts (61 percent versus 57 percent). Thus, it appears that although fewer people in Massachusetts had interest in the demonstration at the beginning of the process, those who completed the first step were almost as likely as their counterparts in Washington to proceed to the next step -- application.

RANDOM ASSIGNMENT

In both projects, applications were reviewed by demonstration staff to ensure timely submission, completeness and that the business ideas satisfied the guidelines of the demonstration. Those applications that met the criteria were then randomly assigned. In Massachusetts, 80.7 percent of all applications submitted were randomly assigned, whereas in Washington 78.0 percent of submitted applications were randomly assigned. As indicated in Table 5.1, in Washington the takeup rate (i.e., the rate of participation among those eligible to participate) was 3.6 percent as compared with only 1.9 percent in Massachusetts. This substantial takeup rate difference in the two demonstrations is due largely to the difference in the Information Session attendance rates.

TIMING OF INTAKE ACTIVITIES

Both demonstrations were intended to be early intervention programs. It was anticipated that by recruiting claimants early in their UI claim, and by providing self-employment training

and counseling services as early as possible, the programs would provide the maximum possible support to individuals during the business startup period.

In the Washington demonstration, early delivery of program services also had an important monetary incentive. That is, demonstration participants were required to complete five project milestones in order to receive a lump-sum payment of their remaining UI benefits. To maximize this amount, it was important to complete the training and develop a business plan as soon as possible.

Table 5.2 shows the average number of days between key intake and service activities in the Washington and Massachusetts demonstrations. The total number of days between benefit year begin date/effective date of claim and attendance at the orientation session was 33 days in Massachusetts and only 18 days in Washington. This difference is largely explained by the fact that Washington, as a wage reporting state, could complete monetary eligibility determination more quickly than Massachusetts, a wage request state. This procedural difference resulted in the observed 15-day difference between the two demonstrations.

Table 5.2
Washington/Massachusetts Comparison
Average Number of Days Between Key Program Activities
(All Treatment Group Members)

Activity	Washington (N=755)	Massachusetts (N=614)
Benefit Year Begin Date/Effective Date of Claim to Information Session/Awareness Day	17.7	32.8
Orientation Session to Random Assignment	11.1	13.4
Random Assignment to Enterprise Seminar/First Training Module	10.2	13.1
Benefit Year Begin Date/Effective Date of Claim to Enterprise Seminar/First Training Module	39.0	59.0

Source: PTS

The elapsed time between the orientation session and random assignment was also slightly longer in Massachusetts as compared with Washington (13 days versus 11 days). Similarly, the elapsed time between random assignment and attendance at the first training sessions was longer in Massachusetts than in Washington (13 days versus 10 days).

The cumulative effect of each of the above delays in Massachusetts relative to Washington is measured by comparing the total elapsed time between the benefit begin date and the date of the first business support activity (Enterprise Seminar/First Training Module). The elapsed time on this measure is 20 days longer in Massachusetts than in Washington (59 days compared to 39 days). Thus, on every measure examined, services were provided more rapidly in the Washington demonstration than in the Massachusetts demonstration.

Table 5.3
Washington/Massachusetts Comparison
Business Assistance Services Received by the Treatment Group
(Percentages based on PTS data)

Business Assistance Services	Washington (N=755)	Massachusetts (N=614)
Business Training Modules		
Attended Enterprise Seminar/Module 1	84.8%	93.3%
Attended At Least Six Bi-Weekly Workshops/ Attended (or Waived) All Modules	83.4%	49.7%
Business Counseling Hours		
No counseling	29.9%	7.3%
Mean Hours of Counseling	1.5	7.5
Number of Entrepreneur Club Meetings Attended		
None	64.1%	NA
Mean Number of Meetings Attended	0.7	NA

Source: PTS

PROGRAM EXPERIENCES

In Table 5.3 we provide information on the receipt of business assistance services by demonstration participants. As shown in the table, a higher percentage of Massachusetts treatment group members (93 percent compared with 85 percent) attended the first activity after random assignment. However, a much smaller proportion of Massachusetts participants attended all the required workshop sessions. In Massachusetts, only approximately half of the treatment group attended all six Enterprise workshops; in Washington, 83 percent of the treatment group attended the four required training sessions. This difference is partly explained by the difference in timing of the workshops in the two demonstrations. That is, in Washington, the four training modules were offered on four consecutive days; in Massachusetts, the six workshops were offered over a period of between nine and twelve weeks. This longer period in Massachusetts provided participants more time to drop out of the demonstration.

The higher completion rate in Washington than in Massachusetts may also reflect the incentive of the lump-sum payment. That is, in Washington, one of the five milestones for receiving a lump-sum payment was the completion of all the training modules. This incentive may, therefore, have inspired some people to complete the training module sequence. In Massachusetts, there was no comparable incentive since there was no lump-sum payment.

Individual counseling sessions were attended more frequently and for a longer time among Massachusetts participants. Only 7 percent of Massachusetts participants did not receive any individual counseling, while the same was true for nearly 30 percent of Washington participants. The average number of counseling hours was 7.5 in Massachusetts compared with 1.5 hours in Washington. This difference may again be attributed to the scheduling of business training services in the two demonstrations.

In Massachusetts, participants attended the Enterprise Seminar, followed by six workshops. The larger number of sessions in Massachusetts, combined with the longer elapsed time over which these sessions were offered, provided Massachusetts participants more contact with their counselors and thus more opportunities to schedule an individual counseling session. The ongoing contact with the counselors over a longer period of time ensured that the counselors

were aware of the participants' progress through each step and thus could schedule counseling sessions when it became apparent that problems might be occurring.

In Washington, the four training modules were scheduled to be completed within a one-week period. In such a short and intense period, participants had little opportunity to schedule a private session with their counselor prior to the completion of the training program. In many cases, participants did not have a scheduled session with their counselors until their milestone review.

Participant Satisfaction with Business Services

In the follow-up survey we asked demonstration participants their opinions of the demonstration business assistance services. Table 5.4 presents information on the opinions of demonstration treatment group members regarding the quality of the services they received. As shown in the table, opinions about the business training sessions were favorable and quite similar in the two demonstrations. Approximately four-fifths of those attending at least one training session rated the sessions as good or excellent. A slightly higher percentage rated their instructors as good or excellent.

As noted previously, a higher percentage of Massachusetts treatment group members attended individual counseling sessions. In both demonstrations, however, participants rated their counseling sessions and their counselors quite favorably. Again, approximately four-fifths had a favorable reaction.

Business Starts by Participants

In Washington, 333 businesses were started by treatment group survey respondents in the first 21 months after random assignemnt. In Massachusetts, 117 businesses were started by treatment group survey respondents during the first 19 months.[30] Based on the number of treatment group survey respondents in each demonstration, on average, each treatment group member started .55 businesses in both Washington and Massachusetts.

[30]The Massachusetts results are based on Cohorts 1 and 2 only.

Table 5.4
Washington/Massachusetts Comparison
Participants' Rating of Program Experiences

Program Experiences	Washington	Massachusetts
Business Training Modules*		
Number who attended at least one business training workshop/Enterprise Seminar or module	508	404
Percentage who rated the business training workshops excellent or good	80.1%	79.2%
Percentage who rated the business training workshop instructors excellent or good	84.1%	84.4%
Individual Counseling Sessions*		
Number who reported attending counseling sessions	268	374
Percentage who rated the sessions excellent or good	83.6%	78.9%
Percentage who rated the business counselors excellent or good	85.5%	82.4%

Source: PTS and Wave I Survey Data

* The responses presented are for individuals who reported attending the activity on the follow-up survey and who also had a PTS record indicating attendance at that activity

Table 5.5 presents the characteristics of the businesses started by treatment group members. For example, the table presents the distribution of industries in which businesses were started, whether the businesses were home-based, the structure of the enterprises, the percentage of the businesses owned by the treatment group member, and type of financing used. The results indicate that in both demonstrations, the service industry is the most common industry group for starting a business. Some examples of these businesses include personal services, accounting, and printing. Moreover, a substantial majority of all businesses started by treatment grup members were home-based; most businesses were sole proprietorship; and most required

the owner to invest his/her own money. We also examined the charateristics of businesses started by control group members (results not shown in table) and found these results to be quite similar to the above findings.

Table 5.5

Massachusetts and Washington
Characteristics of Treatment Group Members'
Self-Employment Enterprises
(Percent)

	Massachusetts	Washington
Industry		
Construction	3%	4%
Manufacturing	6%	8%
Wholesale/Retail	7%	5%
Services	23%	19%
Other	3%	5%
No Self-Employment	58%	60%
Whether Operated From Home		
Home-Base	32%	29%
Not Home-Based	10%	11%
No Self-Employment	58%	60%
Structure of Enterprise		
Sole Proprietorship	34%	33%
Partnership	1%	2%
Corporation	5%	5%
No Self-Employment	58%	60%
Percent of Enterprise Owned		
Own 100%	37%	34%
Share Ownership	3%	6%
No Self-Employment	58%	60%
Financial Investment		
Invested Own Money	38%	37%
Did Not Invest	4%	3%
No Self-Employment	58%	60%

Source: Wave I Survey Data

6

DATA SOURCES

The analysis in this report is based on data from the following sources: the Participant Tracking System (PTS), administrative records, telephone follow-up surveys and demonstration expenditure data. A detailed description of these data was presented in Benus, et al., 1993. Here, we present a brief description of the data sources that are used in the present analysis. First we present a description of the Participant Tracking System and its use in the analysis, followed by a description of the administrative data used in the evaluation. We then describe the two followup surveys conducted for this study.

PARTICIPANT TRACKING SYSTEM

The Participant Tracking System (PTS) is an on-line database system developed by DOL to provide ongoing information about project participants and project services. The PTS served as an integral component of the demonstrations by performing such functions as targeting project participants, generating letters to participants, randomly assigning individuals to treatment and control groups, and maintaining on-line information about project services. In addition to performing all these functions during the project implementation phase, the PTS also provided data for analyzing program operations.

For the present analysis, the PTS provides data on: (1) individual characteristics; (2) demonstration services; and (3) UI payment information. Data on sample members' characteristics are used to describe claimants targeted for the demonstration. These data are also used to compare the characteristics of the treatment and control groups members. Finally, individual characteristics data are used in the impact analysis as control variables in multivariate regressions.

Data on demonstration services are used in the process analyses of the Washington and Massachusetts demonstrations, presented above in Chapters 3, 4, and 5. For example, we used data on participation in demonstration activities to assess the timing and level of participation in key demonstration activities. PTS data on sample members' UI benefit receipt were used to develop UI outcome measures such as total weeks paid, total benefits received, and whether the claimant exhausted benefits.

ADMINISTRATIVE RECORDS

UI Wage Records

A second source of administrative records, available in Washington only, were State UI Wage Records that are reported by employers on a quarterly basis. For the evaluation, we obtained quarterly information on the total wages and hours worked of employees in covered employment during both the immediate pre-program and post-program periods. In particular, we obtained information on wages and hours worked in covered employment for the five full calendar quarters before the individual filed a new claim and entered the demonstration, for the quarter the claim was filed, and for the following nine calendar quarters. These data were used to construct key outcome measures of employment and earnings experiences, as well as control variables.[31] In addition to providing an important source of supplemental employment and earnings outcomes information, the UI Wage Records data also enabled us to determine the Standard Industrial Classification (SIC) code of the employer, as well as whether the claimant returned to work for the same employer after the demonstration.[32]

---

[31] Approximately 10 percent of the quarterly records had missing data on hours worked. In these cases, hours worked were imputed following a two-step procedure. For claimants with missing data in one (or more) of the five pre-experimental quarters, the person's average (real) hourly wage rate was computed during the entire pre-experimental period and applied to the total wages data in the specific period to impute hours worked for that quarter. A similar procedure was used for the nine post-enrollment quarters. For the few claimants for whom we could not compute an average wage (i.e., always had missing hours worked data), we estimated an hourly wage regression equation and applied the predicted values to total wages in the quarter to impute hours worked. The predictor variables for the hourly wage imputation equation included demographic characteristics (e.g., age, age-squared, male dummy, education dummy variables, race/ethnicity dummy variables), site dummies, dummies for pre-program industry and occupation, and quarter and year dummy variables.

[32] Using the employer tax account number for wages reported in each quarter we were able to determine whether claimants returned to work for the same employer and calculate how many quarters they worked for different employers.

UI Wage Records have several potential advantages over survey data for measuring earnings received from wage and salary employment. For example, UI records are not subject to interviewer bias or respondent recall error. Also, these data are not subject to problems that arise from some respondents reporting net (after-tax) earnings, and others reporting gross (before-tax) earnings. Moreover, they are not affected by response-rate problems.

Although the use of UI Wage Records has a number of advantages for the evaluation, it must be recognized that they do not include all earnings received by claimants.[33] In particular, they do not include wages in uncovered employment, earnings from self-employment, or wages earned in other states. In general, self-employed individuals constitute the large majority of uncovered employment. Other types of uncovered employment include casual labor, religious organizations, newspaper deliverers, insurance agents and real estate agents paid on a commission basis only, and barbers/hairdressers.

A more important limitation of using UI Wage Records to develop measures of earnings from wage and salary employment arises for individuals who live near state borders. Because the system is state-based, it is impossible to distinguish individuals who work across the border in a different state from individuals who do not work in covered employment. This is particularly problematic for claimants in the Vancouver site -- who comprise roughly one-quarter of the SEED analysis sample -- and who generally live within a few minutes of the Oregon border and the Portland metropolitan area. To the extent that treatment and control group members find jobs in Oregon, this measurement error problem would bias the net impact of the program on UI wages toward zero.

Because of the advantages of survey data for measuring earnings for all claimants (regardless of their geographical location), as well as in developing more precise measures of pre- and post-SEED wages, the main results presented in Chapter 7 rely on the interview data described later in this chapter. However, we also present an alternative set of impact results based on outcome measures developed from UI Wage Records to determine how sensitive the results are to different measures.

---

[33] Moreover, because they are only available on a quarterly basis, it is difficult to develop precise measures of earnings for the pre- and post-SEED periods. This could be a particular problem if most of the effects of the demonstration were to occur relatively early in the claim spell.

Department of Revenue Data

The third major source of administrative records for the impact evaluation is Department of Revenue (DOR) data (available only in Washington). For the evaluation, we obtained DOR records for individuals in the SEED Demonstration for an 11-year period, 1981-1992. This enabled us to develop several pre-program and post-program indicators of business activity. Below we describe the data obtained, their advantages and potential limitations.

To obtain the DOR data, a file of all demonstration participants -- both treatment and control group members -- was prepared by the SEED unit and sent to DOR for matching purposes. The file contained the person's name, SSN, and when known, the person's Uniform Business Identifier (UBI). The UBI is a unique identification number that is attached to the person's business license. For businesses contained in the DOR file that were matched by DOR to demonstration participants -- treatment group and control group members -- we obtained the following data:

- Date business opened and current status (e.g., whether open/closed);
- Industrial sector;
- Gross income and sales;
- State and local sales taxes paid; and,
- Business and occupation taxes paid.

Because some businesses are required to pay taxes on a monthly basis, while others pay taxes on a quarterly or annual basis, we aggregated the sales and tax data into annual measures for all businesses. We also developed indicators of whether the business was "active" during a year, based on it being open during the year and having positive gross sales or income.

The availability of DOR records provide a valuable supplement to the self-employment information obtained from the follow-up interviews for claimants in Washington State. Of particular importance, they enable one to estimate the impact of the demonstration on State taxes paid, which is a component of the overall benefit-cost assessment. On the other hand, as described below, there are some important limitations to the DOR data that must be recognized.

First, similar to the UI Wage Records problem described above, because the DOR data are state-based, they only include business activity in Washington State. Second, because the DOR data are a direct result of businesses paying various excise taxes on sales and income, and because individuals involved in fee-for-service businesses who receive less that $12,000 annually do not have to pay business and occupation taxes, it is likely that some of this income is unreported. Third, because the data relate to a specific DOR business registration number, and activity from more than one business can be reported under the same tax registration number, it is not generally possible to separate the business activity of a particular individual from that of his or her spouse, parents, siblings, or friends that are also reported on the same form. This makes it difficult to link the sales and tax information to specific treatment and control group members. Moreover, to the extent that SEED participants join businesses with pre-existing tax registration numbers, it greatly limits the usefulness of other data such as business opening and closing dates and industry.[34]

A final concern about the DOR data involves the way in which the DOR records were matched to SEED participants, which could in turn result in potential differences in data quality between treatment and control groups. SEED treatment group members who started a business and received a lump-sum payment had to demonstrate they had a business license. Because the UBI associated with the license is the primary mechanism used by DOR to match businesses, it is possible that the matching process was biased in favor of obtaining relatively more DOR records for treatment group members than for control group members. That is, although the DOR file contains the SSN and name of the person listed as the business owner that can also be used for matching purposes, these data are often of lower quality and could potentially result in fewer matches. However, available evidence indicates that this was not a serious problem for the evaluation.[35]

---

[34] In addition, information in the DOR data base concerning the date a business closed is not likely to be very reliable since business owners may simply choose to report "no activity" on their tax forms for several periods, rather than indicate their business closed.

[35] First, the DOR data indicate no significant differences between treatment and control group members in any pre-program business activity measures. Moreover, based on an earlier pilot study, among individuals with known UBI, we were able to match 85 percent of the businesses based on SSN and name only. Thus, the extent of bias in these data between the two groups is likely to be small.

SURVEY DATA

Two followup surveys were conducted by Abt Associates' Survey Research Group using Computer Assisted Telephone Interviewing (CATI). The first of these follow-up surveys (Wave I) was administered to all Washington and Massachusetts demonstration participants (treatment and control group members) approximately 1.5 to 2 years after random assignment. The second survey (Wave II) was administered approximately one year later to all Washington and Massachusetts Cohort 1 and Cohort 2 participants who responded to the Wave I survey. Only one followup survey (Wave I) was administered to the Massachusetts Cohort 3 participants. The Wave I survey for Cohort 3 was administered to a total of 496 participants[36], those randomly assigned from March 1, 1992 through January 19, 1993.

The elapsed time between random assignment and the Wave I surveys is presented in Table 6.1. As seen in the table, the median survey follow-up period was 18.8 months in Massachusetts as compared with 21.3 months in Washington. Table 6.2 shows the median length of time between the Wave I and Wave II surveys is similar in both demonstrations (11.8 months in Massachusetts and 11.4 months in Washington). Combining both periods, the total observation period in Massachusetts is approximately 31 months and the total in Washington is approximately 33 months.

The administration of the follow-up surveys was complicated by the fact that the Massachusetts demonstration was implemented in three phases and over a three-year period. That is, to ensure a consistent follow-up period for each of the cohorts, the follow-up surveys had to be staggered. The staggered interview periods are depicted in Exhibit 6.1. As shown in the exhibit, Wave I interviews for the Washington sample were between January and April, 1992. Wave I interviews for the three Massachusetts cohorts were administered as follows: for Cohort 1, between January and April, 1992; for Cohort 2, between January and April, 1993; and for Cohort 3, between January and March, 1994.

Wave II interviews were administered between January and April, 1993 for the Washington sample. For Massachusetts Cohort 1, Wave II interviews were also conducted

[36] As discussed in Chapter 4, a total of 701 individuals were randomly assigned during the 1992-1993 Cohort. The analysis sample for this cohort, however, is the 496 with whom we attempted Wave I interviews.

Table 6.1
Survey Follow-up Period
Elapsed Time Between Random Assignment and First Follow-up Interview
(In Months)

Survey Follow-up Period	Massachusetts (N=854)	Washington (N=1204)
Minimum	12.5	17.8
Lowest Quartile	17.4	20.3
Median	18.8	21.3
Third Quartile	20.4	22.4
Maximum	24.2	30.6

Source: Followup Survey

Table 6.2
Survey Follow-up Period
Elapsed Time Between First and Second Follow-up Interview
(In Months)

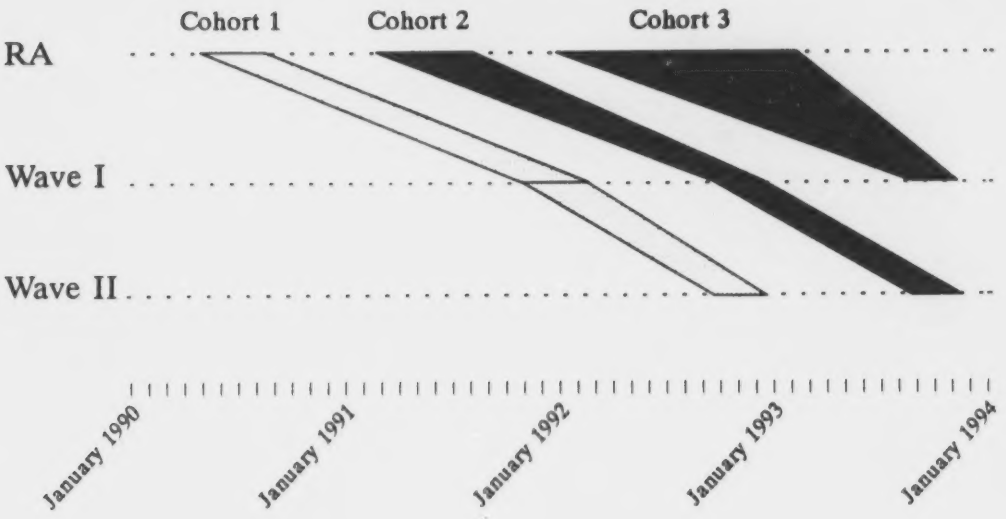
	Massachusetts (N=398)	Washington (N=1087)
Minimum	8.9	8.9
Lowest Quartile	11.6	10.8
Median	11.8	11.4
Third Quartile	12.1	12.0
Maximum	13.5	13.9

Source: Follow-up Survey

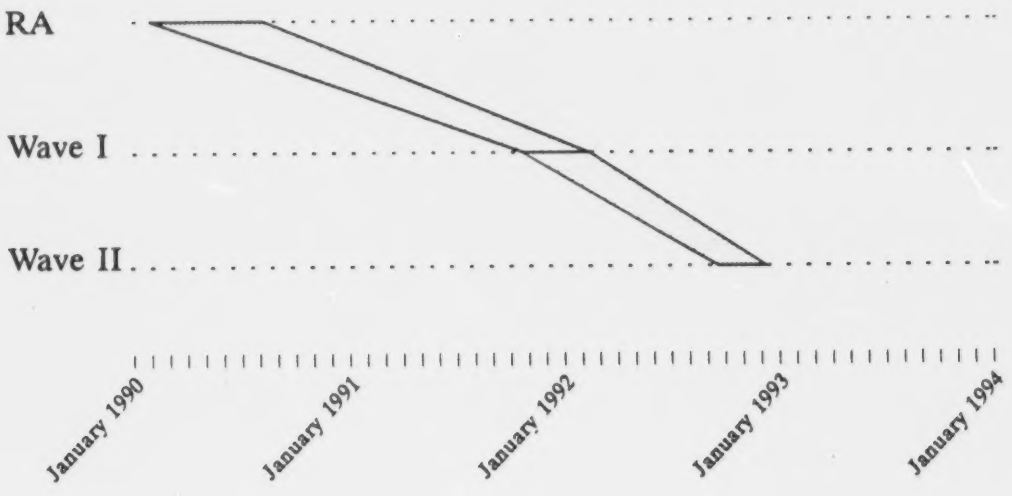
Exhibit 6.1

Timing of Wave I and Wave II Surveys in Washington and Massachusetts

Massachusetts



Washington



between January and April of 1993. For Cohort 2, however, the interviews were conducted between January and April of 1994. There are no Wave II interviews for Cohort 3. The analyses of demonstration outcomes presented in subsequent chapters of this report are based on survey data from both Wave I and Wave II interviews.

Contents of the Survey Data

The follow-up surveys collected detailed pre- and post-program information about employment and earnings. The surveys also collected information on periods of unemployment, periods of time spent looking for work, demographic characteristics, and experiences with the programs. Specific categories of variables that were collected in the surveys are:

Current and Most Recent Spells of Wage and Salary Employment

- Length of each spell
- Average number of hours worked each week
- Salary for each spell
- Industry and Occupation
- Fringe Benefits received and personal satisfaction with the job

Other Spells of Wage and Salary Employment in the Followup Period

- Length of each additional spell of employment
- Salary for each additional spell
- Average number of hours worked each week

All Spells of Self-Employment in the Followup Period

- Industry, product, and organizational structure of the business
- Hours worked per week
- Participation of family members in ownership/operation of business
- Sources of capital used to operate the business
- Business liabilities and assets
- Earnings from the business
- Fringe benefits received
- Personal satisfaction with self-employment
- Number of family members and other individuals employed in the business

Employment Prior to Random Assignment

- Length of longest period of employment since age 18
- Type of employment for that spell (wage and salary or self-employment)
- Number of hours worked per week and salary for that employment spell

Spells of Unemployment During the Followup Period

- Number of spells of unemployment
- Length of spells

Periods of Time Spent Unemployed and Looking for Work

- Amount of time per week spent looking for work
- Type of job sought and desired salary

Background Characteristics

- Primary occupation
- Marital Status
- Household size, family income and assets

Participation in the Demonstration

- Participation in demonstration activities and opinions about the quality of those activities
- Reasons for not opening a business
- Opinions about the most useful components of the program
- Receipt of additional self-employment training other than SEED

Response Rates and Characteristics of Survey Respondents

Trained interviewers attempted to interview all sample members (i.e., individuals who were randomly assigned). It should be noted that in Massachusetts, we did not attempt to interview all of Cohort 3. Due to budget limitations, we limited the interview sample to those sample members who were randomly assigned before January 19, 1993 (there were a total of

Table 6.3
Washington and Massachusetts
Survey Response Rates
Wave I and Wave II

	Total Sample	Wave I Respondents		Wave II Respondents	
		N	%	N	%
Washington	1507	1204	79.9%	1087	90.3%
Massachusetts					
Cohort 1 (1990)	207	177	85.5%	159	89.8%
Cohort 2 (1991)	314	272	86.6%	239	87.9%
Cohort 3 (1992-1993)	496	405	81.7%	NA	NA

Source: Followup Survey

496 such individuals). In Table 6.3 we present the response rates for both Washington and Massachusetts and for each of the waves.

In the Washington demonstration, interviews were attempted with a total of 1507 sample members (755 treatments and 752 controls). Interviews were completed with 1204 (604 treatment group members and 600 control group members), for a response rate of 80 percent. For the Wave II survey, we attempted to interview all of the respondents to Wave I; 1087 (561 treatments and 526 controls) responded to Wave II for a response rate of 90 percent.

In the Massachusetts demonstration, 207 Cohort 1 participants (105 treatment group members and 102 controls) were called for the Wave I survey. A total of 177 Wave I interviews were completed (89 treatments and 88 controls), for a response rate of 86 percent. For the Wave II survey, 159 interviews (82 treatments and 77 controls) were completed, for a response rate of 90 percent.

For Cohort 2 in Massachusetts, a total of 314 Wave I interviews were attempted (158 treatments and 156 controls). We completed 272 Wave I interviews (140 treatments and 132 controls) for a response rate of 87 percent. We also completed 239 Wave II interviews (122 treatments and 117 controls) for a response rate of 88 percent.

For Cohort 3, Wave I interviews were attempted with 496 individuals randomly assigned from March, 1992 through January, 1993 (250 treatments and 246 controls). A total of 405 Wave I interviews were completed (213 treatments and 192 controls), for a response rate of 82 percent.

To assess whether survey respondents differed systematically from the total sample, we compare in Table 6.4 the demographic characteristics of the Wave I respondents with the total sample.[37] As indicated by these results, the respondent sample is similar to the total sample in both Washington and Massachusetts. We found no statistically significant differences between respondents and non-respondents in either the Washington or the Massachusetts samples. We also examined the demographic characteristics of Wave II respondents (not presented here) and found no statistically significant response bias.

In addition to assessing the effect of non-response on the representativeness of the two samples, we also compared the demographic characteristics of the Washington and Massachusetts samples. As indicated in Table 6.4, we found some statistically significant differences between the Washington and Massachusetts samples. The Washington sample has a higher percentage of individuals under age 25 (4 percent in Washington compared with 2.5 percent in Massachusetts). The Massachusetts sample has higher educational attainment, with 44.8 percent of the sample college graduates compared with 28.7 percent of the Washington sample. Massachusetts sample members are more likely to have previously worked in professional/technical/managerial occupations and Washington sample members are more likely to have worked in clerical occupations. Finally, the Massachusetts sample has a higher percentage of African Americans than does the Washington sample.

[37] We did a similar comparison for Waves I and II and found very similar results.

Table 6.4
Characteristics of
Individuals Randomly Assigned and All Survey Respondents

Characteristics	Massachusetts		Washington	
	Total Sample (N=1222)	Wave I Survey Respondents (N=854)	Total Sample (N=1507)	Wave I Survey Respondents (N=1204)
Gender				
Male	69.0%	69.7%	67.3%	65.0%
Age at Random Assignment				
Mean Age (in years)	40.7	40.9	39.5	39.7
Percent Age < 25 **	2.5%	2.0%	4.0%	3.3%
Percent Age > =45	27.9%	29.3%	27.9%	28.3%
Education				
Percent College Graduate ***	44.8%	46.7%	28.7%	29.8%
Mean Education (in years)	14.5	14.6	13.8	13.9
Prior Work Experience				
Professional/technical/ managerial occupation ***	46.6%	47.8%	36.7%	38.3%
Clerical occupation ***	10.0%	9.9%	13.1%	14.0%
Services sector	28.5%	27.5%	27.7%	28.2%
UI Entitlement ($)				
Mean Weekly Benefit Amount	$259	$261	$197	$199
Mean Maximum Benefit Payable	$7723	$7780	$5427	$5516
Race/Ethnicity				
Caucasian	89.2%	91.1%	91.3%	94.5%
African American ***	8.4%	6.9%	3.2%	1.9%
Hispanic	1.5%	.9%	2.1%	1.5%
Other	.9%	1.1%	3.5%	3.0%

Source: PTS

Tests for differences between Massachusetts and Washington samples:

*** Indicates values are significantly different from zero at the .01 level.
** Indicates values are significantly different from zero at the .05 level.
* Indicates values are significantly different from zero at the .10 level.

7

WASHINGTON DEMONSTRATION IMPACTS

In the first part of the report we described the design and implementation of the UI Self-Employment Demonstrations and the data sources available for the analysis. With this chapter, we begin Part II, which focuses on the evaluation of demonstration program impacts on key self-employment and wage and salary job outcomes. Throughout the evaluation presented below, we measure program impacts by comparing the experiences of treatment group members with the experiences of control group members following random assignment.

In this chapter we concentrate on the impacts of the Washington Self-Employment and Enterprise Development (SEED) Demonstration on the self-employment and wage and salary employment experiences of demonstration participants.[38] Before beginning our presentation of program impacts, we first describe the procedures used to estimate program impacts. We then present the detailed results for each outcome measure, one measure at a time; readers who are not interested in all of the details may wish to skip to the summary of the findings at the end of the chapter.

We first describe the program impacts of SEED on self-employment and wage and salary employment measures based on followup survey data. We begin with an analysis of the impacts of SEED on several key measures of self-employment outcomes based on followup survey data, including the likelihood of becoming self-employed, total time in self-employment, the likelihood of being self-employed at the time of the followup surveys, and total earnings from self-employment. We then examine the impact of SEED on several similar measures for wage and salary employment. We then combine self-employment and wage and salary employment and

[38] Throughout the analysis we do not distinguish between self-employment and business ownership. Inasmuch as we do not know who among the self-employed of today may become the large business owner of tomorrow, we choose not to distinguish among individuals who are self-employed and those who operate businesses that employ others. Thus, in our analysis we combine self-employment and business ownership and refer to business ownership as self-employment and vice versa. Later in the chapter, however, we examine the impacts of SEED on number of employees hired.

earnings and examine the impacts of SEED on total employment and earnings outcomes. This section concludes with an examination of the impact of SEED on job creation using survey data.

This is followed by a detailed examination of SEED impacts on various outcome measures derived from administrative data sources available in Washington State. We begin by examining the key issue of SEED impacts on benefit payments received during the benefit year of the demonstration and on subsequent benefit years. We then examine alternative measures of self-employment outcomes based on administrative data sources, including the likelihood of receiving business income, total gross business income received, whether the business was still open, and State business taxes paid. In addition, we examine the impacts of SEED on the number of business employees and on wage and salary earnings based on quarterly UI Wage Records.

Following the presentation of the basic impact estimates on both survey data and administrative records, we describe how the impacts of SEED differ among key claimant subgroups. The chapter concludes with a summary description of the main findings from the impact analyses that are used in the benefit-cost analysis presented in Chapter 10.

In Chapter 8 we present the main demonstration program impact results for the Massachusetts Enterprise Project. Because of limited availability of administrative data sources, most of the impact analyses for the Massachusetts Enterprise Project reported in Chapter 8 rely on the followup survey data. In Chapter 9, we conclude Part II with a comparison of the patterns of results from the two self-employment demonstrations and present additional analyses to better understand the sources of potential differences in the results.

Estimation of Demonstration Impacts

Our measure of demonstration impacts is the difference between treatment group outcomes and what would have happened in the absence of the demonstration, as measured by the outcomes of the control group. For any given outcome, an unbiased measure of demonstration impact is provided by a simple difference in treatment and control group means. We refer to this simple difference in outcome means as the unadjusted program impact. In the tables that follow, we present the simple difference in means in the column labelled *Difference*.

A more precise, and still unbiased, impact estimate can be obtained through multivariate analysis, using covariates to explain some of the variation in outcomes across the sample. By including a variable that captures treatment status (i.e., T=1 if the claimant is in the treatment group and T=0 if the claimant is in the control group), we can obtain an unbiased estimate of the average impact of the demonstration on the outcome by using ordinary least squares (OLS). We refer to impact estimates obtained from such multivariate regression techniques as the regression-adjusted program impact.[39] The regression-adjusted impacts are presented in the tables that follow in the last column labelled *Impact*.

In both the unadjusted and adjusted program impact estimates, a standard t-test can be calculated to determine whether the estimated impact is significantly different from zero. A single asterisk (*) following a given impact indicates statistical significance at the 10% level; a double asterisk (**) indicates significance at the 5% level; and a triple asterisk (***) indicates statistical significance at the 1% level. Impacts with no asterisks are not statistically significantly different from zero at the 10% level. Thus, only estimates that are significantly different from zero at the 10% level or better are treated as evidence of a real effect of the demonstrations.

Some of the outcome variables we investigate are binary in nature. For example, a person either did or did not experience self-employment during the observation period; a person either was or was not self-employed at the time of a follow-up survey. When estimating program impacts on these types of variables, logistic regression is generally considered more appropriate than OLS regression. Although as a practical matter there is often only a small

[39] In addition to a dummy variable for treatment status, all of the regression equations reported in this chapter included age, age-squared, unemployment rate in the claimant's county of residence during 1990, and dummies for the following variables: demonstration sites, quarter in which the claimant's benefit year started, male, white, completed college, prior job in professional, technical or managerial occupation, prior job in services sector, whether the claimant indicated s/he intended to return to work for prior employer on the SEED application, spouse employed, having children under the age of six, having prior work experience related to proposed business, having a business at time of SEED application, having been self-employed before SEED application (but not at the time of application), being a high wage earner (i.e., in the upper quartile) in the four complete quarters before filing the UI claim, and being a medium wage earner (i.e., in the two middle quartiles) in the four complete quarters before filing the UI claim.

difference between the logistic and OLS regression impact estimates, we report the logistic estimates in the tables that examine program impacts on binary outcome variables.[40]

The following tables summarize the demonstration impacts for the Washington SEED Demonstration. The results are presented for two overlapping time periods. First, for each outcome measure, we present the demonstration impact based on data obtained from the Wave I survey, which on average was 21 months after random assignment. In the tables, we refer to these outcomes as the *Wave I* results.

Although the Wave II survey was designed to collect an additional 12 months of data after Wave I, for analysis purposes we combined the data from the two surveys to form a followup observation period that was approximately 33 months after random assignment. In the tables, we refer to such results as *Wave II*, as they are based on the Wave II respondents and cover the entire 33-month observation period, even though they draw on data from both followup surveys.

The tables presented below focus on the overall impacts of SEED, and do not provide information on the claimant characteristics that affect the outcome measures or on whether the impacts of SEED vary by key claimant characteristics. In Appendix B, we present summary information on the effects of covariates that are statistically significant in each of the regression models. We also examined whether the impact of SEED varied by numerous claimant characteristics, including gender, race, site, business experience, and whether the individual intended to return to the previous employer at the time of application to SEED. At the end of the chapter, we describe the general patterns of the subgroup impacts; more detailed information concerning the specific impact estimates for key subgroups is presented in Appendix C.

Self-Employment Impacts

Impacts on Entering Self-Employment. As described earlier, the SEED Demonstration provided business training, counseling, and financial assistance to UI claimants who were both

[40] Because the coefficient estimates from the logistic regressions are not easily interpreted, we transform them into more intuitive figures. Specifically, we transform the treatment/control logit coefficient estimate into an estimate of the impact of the program on the probability of the outcome variable occurring. To perform this transformation, we use the logistic coefficient estimates along with each individual's characteristics to derive an estimated probability of the outcome variable occurring for each sample member. Next, we calculate the average estimated probability for the treatment and the control groups. Finally, we take the difference between these two average probability estimates and use this result as the transformed logit impact estimates.

interested enough in pursuing self-employment to apply to SEED and who were then randomly selected into the treatment group. Given the strong interest in self-employment and the types of services provided to treatment group members, one would expect more treatment group members than control group members to enter self-employment during the observation period. The results in Table 7.1 indicate that, indeed, treatment group members were much more likely than control group members to enter self-employment. Specifically, within the 21-month average observation period corresponding to the Wave I survey, 57 percent of the treatment group, as compared with 32 percent of the control group, were self-employed at some point following random assignment.[41] This 25 percentage point difference in the likelihood of self-employment for the two groups is statistically significant at the .01 level and corresponds to about an 80 percent effect evaluated at the mean of the control group.

As indicated in Table 7.1, by the time of the second followup survey, some additional treatment group members and control group members had a self-employment experience. Over the entire 33-month observation period, 41 percent of the control group, as compared to 63 percent of the treatment group, had at least one self-employment spell. The simple difference estimate of 22 percentage points in the likelihood of having a self-employment experience is slightly lower over the entire 33-month observation period than during the 21-month period of Wave I, but still corresponds to over a 50 percent effect evaluated at the mean of the control group.

Table 7.1

Self-Employment Experience Since Random Assignment

	Control	Treatment	Difference	Impact
Wave I	32%	57%	+25%***	+25%***
Wave II	41%	63%	+22%***	+22%***

[41] The Wave I results reported here differ slightly from the Wave I findings presented in our earlier report (Benus, Johnson, and Wood 1993), although all of the main findings are qualitatively similar. The differences are due to the use of improved definitions of certain outcome measures and slight differences in the analysis samples.

The regression-adjusted impact estimates of the likelihood of entering self-employment reported in the last column of Table 7.1 of 25 percentage points for Wave I and 22 percentage points for the entire 33-month observation period are quite similar to the simple-difference estimates reported in the third column.[42] Thus, we can conclude that the SEED Demonstration had a large positive impact on the likelihood of becoming self-employed, which persisted throughout the observation period.

Impacts on Time in Self-Employment. To analyze the impact of the SEED Demonstration on the time spent in self-employment, we use annualized measures of the number of months in self-employment following random assignment. Specifically, using information on start dates and end dates of self-employment spells from the followup surveys, we calculated annualized months of self-employment as the proportion of the period from random assignment to the interview date that an individual sample member was self-employed multiplied by 12.

As shown in Table 7.2, control group members on average worked at the rate of 1 month per year in self-employment throughout the first and second followup observation periods. Consistent with the results described above, treatment group members spent more time in self-employment on average than controls. In particular, on an annualized basis, treatment group members spent 3.2 months per year in self-employment during the first 21 months after random assignment and slightly less (3.0 months per year) over the entire 33-month observation period.[43] These mean values translate into simple-difference estimates of the impacts of SEED on annualized time in self-employment of 2.2 months per year for the 21-month period and 2.0 months per year for the entire 33-month observation period. These point estimates are the same, regardless of whether the simple-difference estimator or the regression-adjusted approach is

[42] The following covariates are estimated to significantly increase the likelihood of self-employment in both the Wave I and Wave II regression models: being a college graduate, having a business at the time of random assignment, having experience in the area of the proposed business, having a spouse who is employed, and having earnings in the highest quartile in the four quarters before filing the UI claim. For additional details on the effects of the covariates in the impact regression models, see Appendix B.

[43] The lower rate of time in self-employment over the entire 33-month observation period as compared to the 21-month period is partly due to some treatment group members deciding to no longer work in self-employment, as well as a few starting businesses very late and contributing lower than average values on this annualized rate measure.

used.[44] Thus, these results indicate that over the nearly three-year observation period, treatment group members worked approximately 6 months more in self-employment than controls, on average.

Table 7.2

Time in Self Employment Since Random Assignment

	Control	Treatment	Difference	Impact
Wave I	1.0 months	3.2 months	+2.2*** months	+2.3*** months
Wave II	1.0 months	3.0 months	+2.0*** months	+2.0*** months

The greater amounts of annualized time spent in self-employment among treatment group members is partly a result of treatment group members being more likely to have a self-employment spell. In addition, this result could arise if SEED expedited entry into self-employment, or if SEED businesses were more stable and less likely to fail. For example, we might expect treatment group members to start their businesses relatively quickly, as the monetary incentive (i.e., lump-sum payment) they received upon completion of the five business milestones was larger, the sooner the milestones were met. In addition, treatment businesses could be less likely to fail than businesses established by control group members if the demonstration business assistance services were very effective.

To examine these issues, we estimated two sets of hazard rate models for treatment and control group members overall, and for key subgroups. Among those who were not self-employed at random assignment, the first set of models examined the transition time from random assignment to the beginning of the first self-employment spell, where the length of time

[44] Several covariates were statistically significant in both the Wave I and Wave II regression models for annualized time spent in self-employment, including having related work experience in the proposed business area (+), being in the middle two quartiles or in the top quartile of the earnings distribution in the year before filing the UI claim (+) and having an employed spouse (+).

for claimants who never started a business by the time of the survey is treated as censored.[45] The results of these models indicated that treatment group members were significantly more likely to start businesses sooner than controls. This is true over the first five months after random assignment, with the estimated difference in calculated hazard rates being greatest at about 6-8 weeks after random assignment. This is roughly consistent with the timing of the receipt of lump-sum payments described in Chapter 4. Beyond five months, however, the estimated hazard rates of starting a self-employment spell are quite low and virtually identical between treatments and controls.

We also examined whether transition times to beginning a first self-employment spell differed by key subgroups, by estimating hazards rate models with interaction terms for gender, prior income level, and whether the claimant indicated on the SEED application that s/he intended to return to work with the previous employer. Consistent with results described earlier, women are significantly more likely to initiate a self-employment spell sooner than men. We also found that among individuals who indicated they expected to return to work on their SEED application, the estimated hazard rates for treatment and control group members become quite similar after about 45 days. That is, the impact on transition time to self-employment dissipates much quicker for those treatment group members who expected to return to work, and suggest that if they do not start a business soon after random assignment, they are no more likely to return to enter self-employment than control group members with similar expectations. Finally, the results indicate significant differences by prior income level, with treatment impacts on transition time significantly greater throughout the first five months for claimants in the highest quartile of the earnings distribution in the prior year, and smallest for those in the lowest quartile.

The second set of hazard rate models was estimated over the subset of claimants who initiated a self-employment spell. These models examined the transition time from the beginning of the first self-employment spell to the end of a self-employment spell to shed light on the impact of SEED on the likelihood of business failure. Overall, we found that the estimated

[45] The transition probability model incorporated duration dependence through a piece-wise spline, as well as complete treatment interactions with duration.

hazard rates of the risk of ending a self-employment spell for treatment and control group members were quite similar. Although the pattern of estimated hazard rates is consistent with slightly lower business failure rates for SEED treatment group members, the differences were not statistically different from one another. Moreover, because of the very small sample sizes of businesses available for this analysis by subgroup, it was difficult to obtain precise estimates of the transition probabilities for any subgroups and none of the interaction terms were statistically significantly different from zero.

Impacts on Self-Employment at Time of the Followup Surveys. The results described above indicate that treatment group members were more likely than control group members to have a self-employment spell during the observation period, and that they spent more time self-employed than controls. Although these results confirm that the SEED Demonstration was successful in encouraging business startups, they do not necessarily indicate program success since many of the self-employment experiences may not have been successful (i.e., they may have ended up in early terminations). One better indicator of program success is the proportion of individuals remaining in self-employment at the time of the followup surveys.

In Table 7.3, we present information on the proportion of treatment and control group members who were self-employed at the time of each followup survey. As this table indicates, 37 percent of the treatment group were self-employed at the time of the Wave I survey, as compared to 21 percent of the control group. This corresponds to a 16 percentage point difference, which is about a 76 percent effect evaluated at the control group mean. By the time of the Wave II survey, the number of controls in self-employment had increased somewhat (to 25 percent), while the proportion of treatment group members in self-employment had only increased slightly to 38 percent. As a result, the simple-difference estimator yields a 13 percentage point increase in the likelihood of self-employment at the time of the Wave II followup survey, which corresponds to over a 50 percent effect of the control group mean and which is statistically significant. The regression-adjusted impacts for the likelihood of self-employment at the time of the followup surveys are similar to the simple-difference estimates

and provide confirming evidence that SEED increased the likelihood of long-term self-employment.[46]

Table 7.3

Self-Employment at Two Different Observation Times

	Control	Treatment	Difference	Impact
Wave I	21%	37%	+16%***	+16%***
Wave II	25%	38%	+13%***	+12%***

Impacts on Self-Employment Earnings. Measuring earnings for the self-employed is extremely difficult for a number of reasons. First, many new entrants into self-employment do not receive regular income payments from their enterprise. As a result, they may report zero earnings when, in fact, they have positive but irregular earnings. Furthermore, some self-employed individuals underreport their earnings to federal and state authorities (e.g., to illegally reduce their tax liability). These same individuals are likely to underreport earnings in the survey. Still others may not yet have received any earnings from their enterprise (e.g., a contractor who will get paid when a construction job is complete) and, thus, report zero earnings in the survey.[47] For all these reasons, collecting accurate self-employment earnings data is extremely difficult.

In Table 7.4, we present information for self-employment earnings obtained from the Wave I and Wave II surveys. Similar to the time in self-employment outcome measure described above, self-employment earnings are constructed as an annualized measure. Moreover, the earnings figures have been deflated to 1990 dollars using a CPI index. As shown in Table 7.4, annualized self-employment earnings for the entire control group over the Wave I

[46] Some of the same covariates that were statistically significant in the Wave I and II regression models for the likelihood of having a self-employment spell at some point were also significant in both models for the likelihood of being self-employed at the time of the followup surveys. These include dummy variables for being a college graduate and being in the top quartile of the earnings distribution in the year before filing the UI claim.

[47] Results presented in our earlier report confirm the difficulties in measuring self-employment earnings. For example, we found that a large number of self-employed claimants (nearly two-thirds (64.5%) of the respondents) reported zero earnings in the survey.

observation period averaged $695 in 1990 dollars.[48] In contrast, annualized self-employment earnings at Wave I for the treatment group averaged $2,382. The unadjusted impact of $1,687 is significant at the .05 level and the regression-adjusted impact of $1,596 is significant at the .10 level.[49]

The findings over the entire 33-month observation period are also quite similar. In particular, the unadjusted annualized estimate is $1,774 and the regression-adjusted estimate is $1,675. Thus, we conclude that treatment group members earned significantly more per year from self-employment as a result of the program.[50] Over the entire 33-month observation period, this amounts to about $4,600, and it appears that this difference is quite stable and could persist into subsequent years at the same rate.

Table 7.4

Earnings from Self-Employment Since Random Assignment

	Control	Treatment	Difference	Impact
Wave I	$695	$2,382	+$1,687**	+$1,596*
Wave II	$576	$2,350	+$1,774***	+$1,675**

Wage and Salary Employment Impacts

The results described above indicate that the SEED Demonstration had significant positive impacts on the self-employment experiences of program participants. Below, we continue our focus on employment and earnings impacts, but shift our attention to examine the

[48] This average earnings figure includes cases where self-employment earnings were reported to be zero.

[49] The following covariates are estimated to significantly increase self-employment earnings in both the Wave I and Wave II regression models: having a business at the time of random assignment, having experience in the area of the proposed business, having a spouse who is employed, and having earnings in the highest quartile in the four quarters before filing the UI claim.

[50] This analysis included one case (a treatment group member) who reported earning more than $500,000 over the 21-month observation period. The decision to include the case was made in part because the administrative records available in Washington State were unable to convincingly demonstrate that the survey data were incorrect. The estimated self-employment earnings impacts for Wave I (II) are $1,042 ($1,123) when this case is excluded; both effects are statistically significantly different from zero.

impacts of the demonstration on the wage and salary employment experiences of program participants.

SEED could affect wage and salary outcomes for a number of reasons. For example, suppose that the demonstration assisted the more capable treatment group members in pursuing self-employment; less capable treatment group members would likely remain unemployed or be employed in wage and salary jobs. In this instance, we would find a negative program impact of SEED on wage and salary earnings.

Similarly, if the demonstration delayed entry into wage and salary employment for some treatment group members, their wage and salary earnings would likely be lower than they otherwise would have been. For example, if some treatment group members participated in the business training and began developing a business plan for several weeks before recognizing that self-employment was not for them, they are likely to have postponed their search for wage and salary employment during this interval compared to control group members who were not delayed in their search for wage and salary employment. A comparison of wage and salary earnings of all treatment and control group members would also yield a negative program impact for this reason.

On the other hand, if the demonstration services (e.g., business counseling, peer group meetings) provided treatment group members with increased awareness of their marketable skills and enhanced their self-confidence and employability, SEED could have a positive effect on wage and salary earnings. Thus, even though SEED was designed to enhance the self-employment outcomes of program participants, it is likely to have significant effects on the wage and salary experiences of its participants. It is important to examine these effects on wage and salary outcomes to get a sense of the total impact of the program on employment and earnings outcomes, which is the summary measure used in the benefit-cost analysis. We now turn to an analysis of the impacts of SEED on wage and salary employment and earnings outcome measures.

Impact on Likelihood of Wage and Salary Employment Experience. In Table 7.5 we examine the impacts of SEED on the likelihood of having a wage and salary employment experience during the observation period. These results indicate that a smaller proportion of the treatment group than the control group had at least one wage and salary job during the

observation period. By the time of the Wave I survey, 73 percent of the treatment group and 80 percent of controls had at least one wage and salary job, and the difference between the groups is statistically significant at the .01 level. Moreover, the results in the second row indicate that over the entire 33-month observation period, 87 percent of control group members had at least one wage and salary job as compared to 81 percent of the treatment group. This difference of -6 percentage points is also significant at the .01 level, indicating that SEED had a negative impact on the likelihood of wage and salary employment.

Table 7.5

Wage and Salary Employment Experience Since Random Assignment

	Control	Treatment	Difference	Impact
Wave I	80%	73%	-7%***	-7%***
Wave II	87%	81%	-6%***	-6%***

In the fourth column of Table 7.5 we present the results of logit regression models for the probability of having a wage and salary job during the observation period. The same covariates that were used in the regression models for self-employment outcomes described above were used in these regressions.[51] These results confirm our earlier conclusion that the SEED Demonstration had a significant and negative impact on the likelihood of having a wage and salary job during the observation period. The regression-adjusted impact indicates that, on average, the demonstration decreased the likelihood of a wage and salary job for treatment group members by 7 percentage points during the 21-month observation period and by 6 percentage points during the 33-month observation period.

Impact on Time in Wage and Salary Jobs. In Table 7.6, we report the impacts of SEED on annualized months in wage and salary jobs during the observation period. As this

[51] As reported in Appendix B, holding other variables constant, three factors have a statistically significant impact on the likelihood of having a wage and salary job in both observation periods: being a current business owner at the time of SEED application (-), whether the claimant expected to be recalled to his/her prior employer at the time of the SEED application (+), whether the claimant had work experience in the area of the proposed business (-) and whether s/he had earnings in the top quartile in the year prior to filing the UI claim (-).

table indicates, during the first 21 months after random assignment, treatment group members worked 4.1 months per year in wage and salary jobs compared to 4.7 months per year for controls. The reduction of .6 months per year is statistically significant at the .05 level.

The results are quite consistent over the entire 33-month observation period. In particular, the annualized rate of time spent in wage and salary jobs by treatment and control group members increased slightly, such that the reduction in annualized months in wage and salary jobs due to SEED remained about the same (i.e., .7 months per year reduction in time in wage and salary jobs). Thus, over the entire 33-month period, we estimate that SEED reduced total time in wage and salary employment by about 2 months. The regression-adjusted impacts shown in the last column of Table 7.6 are quite similar to the simple-difference estimates and are also statistically significant at the .05 level.[52]

Table 7.6

Time in Wage and Salary Employment Since Random Assignment

	Control	Treatment	Difference	Impact
Wave I	4.7 months	4.1 months	-0.6** months	-0.6** month
Wave II	5.2 months	4.5 months	-0.7** months	-0.7** months

Impact on Wage and Salary Employment at Time of the Followup Surveys. In Table 7.7, we provide information on the proportion of sample members who were employed in wage and salary jobs at the time of the two followup surveys. As this table indicates, at the Wave I followup survey, 60 percent of control group members and 53 percent of treatment group members were employed in wage and salary jobs. This difference of 7 percentage points

[52] Covariates that were statistically significant in both the Wave I and Wave II regression models for time in wage and salary employment include whether the claimant was male, age and age-squared, and whether the claimant was in the two middle quartiles of the prior earnings distribution. Specifically, the results indicate that holding other factors constant, males spent less time in wage and salary employment than females, that the time spent in wage and salary employment increased with age but at a decreasing rate, and that those with medium earnings were more likely to spend time in wage and salary employment than those in the lowest quartile of earnings.

is the same as the regression-adjusted estimate and both are statistically significant at the .05 level. These results reinforce the earlier pattern of findings on the likelihood of having a wage and salary job in the initial 21-months after random assignment. That is, just as we found that treatment group members were less likely than control group members to have a wage and salary job in the 21-months after random assignment, these results indicate that treatment group members are less likely to have a wage and salary job at the time of the first followup survey.

Table 7.7

Wage and Salary Employment at Two Different Observation Times

	Control	Treatment	Difference	Impact
Wave I	60%	53%	-7%**	-7%**
Wave II	61%	57%	-4%	-3%*

By the time of the Wave II survey, however, the difference in the likelihood of wage and salary employment is much reduced. Specifically, at Wave II, 57 percent of treatment group members were employed in wage and salary jobs, as compared to 61 percent of controls. Moreover, an important distinction is that the difference between treatment and control group members at Wave II was not statistically significant at conventional levels. As seen in Table 7.7, the regression-adjusted Wave II impact of -3 percentage points is statistically significant at the .10 level.

Impact on Wage and Salary Earnings. Measuring wage and salary earnings from survey data is less complicated than measuring self-employment earnings. Generally, those in wage and salary employment are paid regularly, their earnings are reasonably stable from month to month, and they are more likely to know their earnings than the self-employed. For all these reasons, we had substantially fewer complications in accurately measuring wage and salary earnings than in measuring self-employment earnings. Consistent with our measure of self-employment earnings, earnings in wage and salary employment are expressed in annualized terms and measured in constant 1990 dollars.

The results for annualized earnings in wage and salary employment are shown in Table 7.8. These results indicate that during the initial 21-month observation period, treatment group members earned $8,029 per year on average as compared to $9,231 per year for control group members.[53] This yields an unadjusted impact of -$1,202, which is not significant at conventional significance levels. However, after controlling for other factors that affect earnings, the regression-adjusted impact of -$1,407 is significant at the .10 level.

Table 7.8

Earnings from Wage and Salary Employment Since Random Assignment

	Control	Treatment	Difference	Impact
Wave I	$9,231	$8,029	-$1,202	-$1,407*
Wave II	$9,920	$8,414	-$1,506*	-$1,780**

The results for the entire 33-month observation period are similar. As described in Table 7.8, average annualized earnings in wage and salary employment for the treatment group over the entire observation period was $8,414, as compared to $9,920 for the control group. This unadjusted difference of -$1,506 is statistically significant at the .10 level. Moreover, after controlling for other factors that affect earnings, the regression-adjusted annualized impact of SEED on wage and salary earnings is -$1,780, which is statistically significant at the .05 level. Thus, we conclude that SEED had a negative impact on the annual earnings of participants from wage and salary jobs during the observation period.

Total Employment and Earnings Impacts

The results described above indicate that the SEED Demonstration had positive and statistically significant impacts on a number of self-employment outcomes, but negative impacts

[53] This average annualized earnings figure includes some cases where wage and salary earnings were reported to be zero. To the extent that some of the zero responses are likely to be misreported, the above average earnings figure is likely to understate actual wage and salary earnings. However, this data problem is much less serious than for self-employment earnings.

on several wage and salary outcomes. The overall impacts of SEED on the combination of these two types of employment experiences are, therefore, ambiguous. In this section, we present our findings of the impact of the SEED Demonstration on combined measures of employment and earnings outcomes.

Impacts on Likelihood of Employment. The results presented above indicate that SEED had a positive impact on the likelihood of having at least one self-employment experience in the observation period and a negative impact on the likelihood of having at least one wage and salary job. In Table 7.9, we examine the impacts of SEED on the likelihood of having at least one employment experience during the observation period, either self-employment or a wage and salary job. These results indicate that during the initial 21-month followup observation period, SEED had a significant and positive impact on the likelihood of employment. Specifically, 96 percent of the treatment group was employed in either wage and salary or self-employment during this period, as compared to 92 percent of controls. The unadjusted and the regression-adjusted impact estimates of 4 percentage points are both statistically significant at the .01 level. Thus, it appears that SEED facilitated early reemployment in general by increasing the likelihood of having a self-employment experience in the initial 21-month observation period.

Table 7.9

Combined Employment Experiences Since Random Assignment

	Control	Treatment	Difference	Impact
Wave I	92%	96%	+4%***	+4%***
Wave II	97%	98%	+1%	+1%

Over the entire 33-month observation period, however, this short-term effect disappears as virtually all of the sample members had held a job of some type during the period. Specifically, 98 percent of the treatment group and 97 percent of the control group held a job or were self-employed at some point in the 33-month observation period. The difference is not statistically significant at conventional levels; the regression adjusted impact estimate is also insignificant.

Impacts on Combined Time in Wage and Salary Employment and Self-Employment. The results described earlier indicate that SEED significantly increased the amount of time per year spent in self-employment, but decreased the time per year in wage and salary jobs. To examine the overall effects of SEED on time in either wage and salary or self-employment, we created an annualized measure of combined months employed in any type of job. The total measure is not simply the sum of the separate annualized measures of time in self-employment and time in wage and salary employment.[54] Rather, the combined measure takes into account information on the dates of each employment experience to avoid double-counting periods during which individuals are working at both self-employment and wage and salary employment.

The results for annualized total time in employment are presented in Table 7.10. For the entire control group, the average annual time in either wage and salary employment or self-employment during the initial 21-month observation period was 5.9 months. Thus, control group members were employed at some job roughly one-half of the initial 21-month observation period. This compares to 7.1 months per year for the treatment group. The unadjusted impact of 1.2 months is statistically significant at the .01 level, as is the regression-adjusted impact estimate of 1.3 months.

Table 7.10

Total Time in Employment Since Random Assignment

	Control	Treatment	Difference	Impact
Wave I	5.9 months	7.1 months	+1.2*** months	+1.3*** months
Wave II	6.4 months	7.5 months	+1.1*** months	+1.1*** months

The results are similar over the entire 33-month observation period. The annualized time in employment increased for both the treatment and control groups -- to 7.5 months and 6.4

[54] As a result, the overall impacts of SEED on this measure do not equal the simple sum of the impacts on time in self-employment and time in wage and salary employment. Moreover, the combined results are not equal to the sum of the separate impacts because of differences in sample size due to missing data on one or the other components.

months, respectively -- resulting in a simple-difference estimate of the impact of SEED of 1.1 months per year. The corresponding regression-adjusted estimate was 1.1 months per year, which was also statistically significant at the .01 level. Thus, we conclude that the SEED program unambiguously increased total time in employment by approximately one month per year, or nearly 3 months over the entire observation period.

Impacts on Employment at Time of the Followup Surveys. The results of the previous sections indicate that SEED increased the likelihood of being self-employed at the time of the followup surveys, but slightly reduced the likelihood of being employed in a wage and salary job at the followup surveys. In Table 7.11, we examine the impact of SEED on the likelihood of being employed in either type of employment at the time of the followup survey.

Table 7.11

Employment at Two Different Observation Times

	Control	Treatment	Difference	Impact
Wave I	75%	78%	+3%	+3%
Wave II	78%	84%	+6%**	+5%**

At the time of the Wave I survey, 75 percent of the control group and 78 percent of the treatment group were employed in either self-employment or a wage and salary job. This simple difference of 3 percentage points, and the regression-adjusted impact of 3 percentage points, are not statistically significant at conventional levels.

This is in contrast to the findings at the end of the 33-month period. Specifically, 84 percent of treatment group members were employed in either self-employment or wage and salary employment at the time of the Wave II survey, as compared to 78 percent of controls. This simple difference of 6 percentage points was statistically significant at the .05 level. Controlling for other factors that also affect the likelihood of employment results in a regression-

adjusted impact of SEED of 6 percentage points, which is significant at the .05 level. Thus, it appears that SEED has long-run impacts in facilitating claimants' likelihood of reemployment.[55]

Impacts on Combined Employment at Time of the Followup Surveys. The analyses described above examine the impacts of SEED on the likelihood of self-employment or wage and salary employment at the time of the followup surveys. Although such analyses provide a valuable overall summary about the impacts of SEED on the likelihood of reemployment, they do not provide any information about the extent to which claimants are involved in both types of employment simultaneously. To more fully understand the impacts of SEED and the experiences of claimants in pursuing self-employment, it is important to understand the extent to which individuals were only pursuing self-employment, only pursuing wage and salary employment, or whether they were employed in both activities simultaneously. In this section, we provide information on this issue.

To address this issue, we divided employment status at the time of the followup surveys into four states: (1) not employed; (2) employed only in a wage and salary job; (3) self-employed only; and (4) employed in both wage and salary and self-employment. In Table 7.12, we provide simple descriptive statistics on these four alternative employment states. As this table indicates, at the time of the Wave I followup survey, approximately 12 percent of the treatment group was employed in both wage and salary jobs and self-employed as compared to 6 percent of the control group. In addition, about 25 percent of the treatment group was only self-employed at the time of the first survey, as compared to 15 percent of the control group. Thus, over one-third of the overall 15.6 percent difference in the likelihood of self-employment at Wave I was due to treatment group members being more likely to be employed in both wage and salary jobs and in self-employment.

[55] Only the prior earnings covariates consistently affect the likelihood of being employed at both followup survey points. Specifically, claimants in the middle and top quartiles of the earnings distribution in the year before filing a claim are more likely to be employed at the time of the followup surveys than other individuals.

Table 7.12

Combined Employment States at Different Observation Times

	Control	Treatment	Difference
Wave I			
Not Employed	25.2%	21.7%	-3.5%
Employed Only in Wage and Salary	53.5%	41.4%	-12.1%
Self-Employed Only	15.0%	25.0%	+10.0%
Employed in Both	6.3%	11.9%	+5.6%
Wave II			
Not Employed	21.9%	15.9%	-6.0%
Employed Only in Wage and Salary	53.2%	46.7%	-6.5%
Self-Employed Only	16.9%	26.6%	+9.7%
Employed in Both	8.0%	10.9%	+2.9%

The results at the time of the Wave II survey are presented in the bottom four rows of Table 7.12. For the most part, the employment patterns at the time of the Wave II survey are quite similar to the Wave I patterns. For the control group, a similar proportion are employed only in wage and salary jobs, and the smaller overall number who are not employed are balanced by a similar increase in the proportion who are self-employed only or who are employed in both wage and salary employment and self-employment. Among treatment group members, the proportion in self-employment only and the proportion in both types of employment are quite similar to the Wave I levels; the reduction in the proportion not employed from Wave I to Wave II is primarily accounted for by an equal increase in the proportion employed only in wage and salary jobs.

To determine the impact of SEED on this joint distribution of employment states, we use a multinomial logit model.[56] The results of the multinomial logit model are summarized in Table 7.13. For ease of interpretation, in this table we report the program impacts on the estimated odds ratios of each employment state relative to not being employed at the time of the followup survey. An odds ratio is the simple ratio of two probabilities, and thus represents the relative likelihood of one alternative to another. In this application, we are examining the impacts of SEED on the relative likelihood of three different employment states to the alternative of not working at the time of the followup surveys. If the odds ratio is less than 1, this indicates that SEED reduced the likelihood of the particular alternative relative to not working; if the ratio is greater than 1, it indicates the program increased the relative likelihood of the alternative relative to not working.

The results in Table 7.13 are quite similar both qualitatively and quantitatively across the two followup surveys and indicate that SEED has a large and statistically significant impact on the odds in favor of self-employment only relative to not being employed, and of both self-employment and wage and salary employment relative to not being employed; the results indicate that SEED did not affect the likelihood of working in wage and salary employment only relative to not working at all. Moreover, the size of the estimated coefficients are reasonably similar across these two employment states and across survey waves. In particular, the odds in favor of being self-employed only relative to not working are about twice as high for treatment group members than for controls in both survey periods. The results also indicate that the estimated odds in favor of working both in wage and salary employment and self-employment are larger for treatment group members than for control group members in Wave I (2.4) than in Wave II (1.9). This indicates a strong long-term positive impact of SEED on the likelihood of working in self-employment only (relative to not working) and a very large short-term effect on working

[56] The multinomial logit model for this application can be written as:

$$\log [P_{ij}/P_{i1}] = X_i b_j + cT_i \, , \; j=2,3,4; \; i=1,\ldots,N,$$

where P_{ij} is the probability that employment status j is chosen by individual i; X_i are the same set of independent variables for individual i that were included in the impact regression models described earlier; T_i is a treatment dummy variable representing the treatment status for individual i; and b_j and c are coefficients to be estimated. To obtain identification, we normalize against the group who are not employed (status of $j=1$) at the time of the survey and impose the constraint that $b_1=0$. Given this constraint, c can be interpreted as the impact of SEED on the logarithm of the ratio P_j/P_1. That is, c is the impact of SEED on the logarithm of the odds in favor of being in employment status j relative to not being employed.

in both wage and salary and self-employment, which declines somewhat over time, although it is still reasonably large and statistically significant at Wave II.

Table 7.13

Multinomial Logit Estimates of the Impacts of SEED on the Odds of Combined Employment Status at the Time of the Followup Surveys

Survey Wave and Employment Status	Impact on Odds Ratio
Wave I	
Employed Only in Wage and Salary	0.86
Self-Employed Only	1.97***
Employed in Both	2.41***
Wave II	
Employed Only in Wage and Salary	1.08
Self-Employment Only	2.12***
Employed in Both	1.93***

Impacts on Total Annualized Earnings. The final results in this section concern total earnings over the observation period from both wage and salary jobs and self-employment. As shown in Table 7.14, average annualized combined earnings from both wage and salary employment and self-employment over the 21-month observation period for the control group was $10,499. Total annualized earnings over the same period for the treatment group averaged $11,477. The unadjusted annualized impact of $978 and the regression-adjusted impact of $566 are not significantly different from zero at the .10 level.

The results for the entire 33-month observation period are broadly similar. Specifically, although the annual rates of earnings are slightly higher -- $11,590 for controls and $12,585 for the treatment group -- the simple-difference estimate of $995 per year is very similar to the Wave I estimate. The regression-adjusted impact estimate of $289 is considerably smaller,

although neither point estimate is significantly different from zero at the .10 level.[57] Thus, we cannot conclude that SEED had any impact on annualized combined earnings from self-employment and wage and salary employment.

Table 7.14

Combined Earnings from Employment Since Random Assignment

	Control	Treatment	Difference	Impact
Wave I	$10,499	$11,477	+$978	+$566
Wave II	$11,590	$12,585	+$995	+$289

Other Demonstration Impacts

In addition to providing employment and earnings for the business owner, small businesses often generate wage and salary employment for others. Frequently, when small business owners need employees they hire family members. In addition, small businesses also generate employment for nonfamily members. In this section, we analyze the demonstration impacts on total nonparticipant employment, where nonparticipant employment includes all family and nonfamily wage and salary employees of the business, excluding the demonstration participant.

We measure the effect of the demonstration on nonparticipant employment as the difference between the mean number of employees per treatment group member and the mean number of employees per control group member. Since this analysis is conducted over all treatment and control group members (whether they have a business or not), those individuals without a business are assumed to have no employees.

As indicated in Table 7.15, SEED did not significantly increase the employment of nonparticipants during the first 21-months of the demonstration, but did have significant impacts over the entire 33-month observation period. Specifically, at Wave II, treatment group members overall averaged .42 nonparticipant employees, as compared to .17 nonparticipant employees for

[57] The much lower program impact estimate obtained from the regression model is surprising and requires further examination.

the control group overall. This difference of .25 nonparticipant employees per treatment group member is statistically significant at the .01 level. This is consistent with the large differences described above between treatment group members and controls in the likelihood of starting a business, the time spent in self-employment, and differences in the earnings from self-employment.

Table 7.15

Job Creation Impacts per Members: Total Employment

	Control	Treatment	Difference	Impact
Wave I	0.22	0.30	0.08	0.07
Wave II	0.17	0.42	0.25***	0.25***

The above job creation impacts may be more easily understood by converting the "per member" job creation impacts to total job creation impacts. The total number of jobs attributable to the SEED Demonstration may be derived as follows. Since control group members employ, on average, .17 nonparticipant employees (see Table 7.15), the entire control group, is estimated to employ approximately 128 nonparticipants (i.e., .17 jobs per control group member * 755 control group members in the demonstration). In contrast, the entire treatment group is estimated to employ approximately 316 nonparticipants (i.e., .42 jobs per treatment group member * 752 control group members). The treatment/control group difference is 188 jobs (i.e., 316-128). The SEED Demonstration, therefore, created a total of 188 nonparticipant jobs during Wave II.

Impact Results Using Administrative Data

The impact results described above examine various measures of self-employment and wage and salary employment and earnings using data from the two followup surveys. In the remainder of this chapter, we supplement these findings by providing estimates of the impact of SEED on several similar outcome measures developed from administrative data sources available in Washington State, as well as examine the program impacts on total benefits

received. Although we regard the impact results based on the survey data discussed above as the best estimates of SEED impacts on employment-related outcomes, the results described below provide valuable additional evidence concerning SEED impacts on similar outcome measures, as well as impacts on other important outcome measures not directly available through the followup surveys.

It is important to note that the results reported below are presented in a somewhat different format than the employment and earnings impacts based on survey data described above. First, because the time periods for which the administrative data were available do not correspond to the times that the surveys were administered, we present the results below for somewhat different followup periods. Second, because the data were obtained for specific calendar periods (i.e., quarters, years), it was straightforward to estimate the impact of SEED on separate postprogram calendar periods. Thus, rather than combine the administrative records across multiple periods and then estimate annualized impacts from several periods of data, we instead estimate separate impacts for several distinct postprogram periods. This provides additional information concerning the time pattern of program impacts. For most measures, one can obtain an annualized estimate over the entire observation period similar in concept to the findings presented for Wave I and II above by summing the estimated impacts over the separate periods and converting it to an annual rate.

The remainder of the chapter is organized as follows. First, we present our analysis of the impacts of SEED on UI benefit payments for the benefit year in which the claimants were enrolled in the demonstration, as well as for subsequent years. We then present average impacts of SEED on several self-employment outcome measures based on Washington State Department of Revenue (DOR) administrative records. The chapter concludes with an examination of UI Wage Records data to estimate the impacts of SEED on the employment and earnings of participants in covered wage and salary employment and on employees hired and wages paid to employees in the businesses started by treatment and control group members.

SEED Impacts on UI Outcome Measures

A central issue in the UI Self-Employment Demonstrations concerns the impacts of the program on the receipt of total benefits. In the interim report, we described in detail the impacts of SEED on several measures of UI benefit receipt during the benefit year corresponding to the

UI claim that lead to enrollment in SEED. Below we briefly summarize those results and provide new findings related to the receipt of UI benefits in subsequent years.

Length of First UI Spell. Using data from the UI history files in the PTS, we developed a measure of the number of weeks of the first spell of UI receipt.[58] As shown in Table 7.16, controls experienced first spells of UI benefits of 17.5 weeks on average. In contrast, the length of the first UI spell for treatment group members was only 11.4 weeks. This leads to simple difference estimates and regression-adjusted impact estimates of a 6.1 week reduction in first UI spell, which is statistically significant at the .01 level. The large reduction in length of first spell is primarily due to treatment group members completing their five milestones to receive their lump-sum payment and then no longer drawing regular UI payments.

Table 7.16
Length of First UI Spell (Weeks)

Control	Treatment	Difference	Impact
17.5	11.4	-6.1***	-6.1***

UI Benefit Payments Received. To examine both the immediate and total effects of SEED on UI payments during the benefit year, we calculated both the UI payments received during the first UI spell and the total UI payments received during the entire benefit year, with and without the lump-sum payment. As summarized in Table 7.17, these results indicate that controls received an average of $3,368 during the first spell. Because controls received $3,792 on average over the entire benefit year, it is clear that the large majority (nearly 90 percent) of the UI payments received during the year occurred during the first spell. The unadjusted and regression-adjusted impact estimates are very similar for both measures and indicate that, excluding the lump-sum payment, SEED had a significant impact in reducing the amount of UI

[58] The first spell of UI benefits is conceptually defined as weeks of continuous receipt of UI payments, and does not include the waiting week. Moreover, in conceptually attempting to link the end of a spell with the beginning of a job, we do not consider a spell to have ended if there is a minimal break in receipt of payments after which respondents continue to receive UI benefits. Because Washington State uses a bi-weekly payment system, we decided to concatenate spells that had breaks of two weeks or less in payments and treat them as a single spell of length equal to the number of weeks with positive payments. In this way, we ensure that claimants who receive a UI payment every two weeks have not ended their spell.

benefits received by claimants. For example, we estimate that SEED reduced the UI benefit payments received during the first spell by $1,270. Moreover, given the estimated program impact of -$1,448 over the entire benefit year, the results indicate that the large majority of the impact on UI benefits paid occurred during the first spell.

As described in Chapter 3, a total of 451 treatment group members completed their required project milestones and obtained a lump-sum payment. The average lump-sum payment for these 451 claimants was $4,225. Although these lump-sum payments were made with federal research funds -- and not from the UI Trust Fund -- it is important to understand what the total impact would be if these payments were funded by the same funding source. As shown in the third row of Table 7.17, taking into account both UI benefit payments plus lump-sum payments, treatment group members are estimated to have received $1,098 *more* on average than controls. Thus, there would be a net cost in the short-term of about $1,000 per claimant for operating a self-employment program like SEED that included lump-sum payments.

Table 7.17

UI Benefit Payments Received During Initial Benefit Year

Time Frame	Control	Treatment	Difference	Impact
First Spell	$3,368	$2,086	-$1,282***	-$1,270***
Full Benefit Year	$3,792	$2,333	-$1,459***	-1,448***
Full Benefit Year Including Lump Sum	$3,792	$4,857	$1,065***	$1,098***

A related issue concerns the extent to which SEED was able to simulate a policy of cashing out UI benefits. As described in Chapter 2, although the lump-sum component of the SEED Demonstration was intended to simulate a cash-out of UI benefits, because UI is an entitlement program and these benefits could not be denied for demonstration purposes, it was

not strictly possible to test a cash-out policy. Operationally, this meant that participants could return to the regular UI program after receiving their lump-sum payment and draw the remainder of their UI entitlement in the form of bi-weekly payments provided they met the normal UI eligibility requirements, including the work search requirement. Our results indicate that, for the most part, SEED can be considered to have tested a cash-out policy. Specifically, only 6.9 percent of the treatment group members who received a lump-sum payment (4.1 percent of the entire treatment group) returned to UI after receiving the lump-sum payment to draw additional UI payments from their original claim. Among those who returned to UI, the average additional UI benefits received was $1,839. This corresponds to about $75 per claimant over the entire treatment group.

Receipt of Subsequent UI Benefit Payments. In addition to examining impacts on UI payments received during the benefit year, it is also important to understand whether SEED affects subsequent UI benefit payments. To investigate this issue, we obtained data from SEED program staff on UI benefit payments received by all treatment and control group members through the first half of 1994. These results are summarized in Table 7.18.

The first row of the table provides information on the proportion of demonstration sample members who file a subsequent claim for a new benefit year and receive at least one payment from the new claim. Our results indicate that 31.8 percent of all controls filed a claim for a new benefit year after SEED. This compares to 31.0 percent of all treatment group members, resulting in a very small difference that is not statistically significant.

The second row reports the average amount of subsequent UI benefits received. As a group, controls received $1,296 in regular UI benefit payments on average from claims filed during the (roughly) three year period after SEED. This compares to $1,214 on average for treatment group members. The regression-adjusted impact estimate of -$85 is not statistically significantly different from zero. Thus, SEED did not affect receipt of subsequent UI benefits.

In the last row of Table 7.18 we report the impact of SEED on total UI payments received over both the initial UI claim and the subsequent claims observed, and including the lump-sum payment. As shown in the last column, our results indicate that the program increased the UI benefits received by $1,013. As described above, all of the estimated increase occurred during the initial benefit year.

Table 7.18

Receipt of Subsequent UI Benefit Payments

UI Measure	Control	Treatment	Difference	Impact
Whether Receive UI Benefits After SEED	31.8%	31.0%	-0.8%	-0.8%
Amount of UI Benefits Received After SEED Year	$1,296	$1,214	-$82	-$85
Total UI Benefits Received	$5,088	$6,071	$983***	$1,013***

SEED Impacts on Self-Employment Outcome Measures

The Department of Revenue (DOR) in the State of Washington maintains extensive information on individual businesses that operate in the State and these data were linked to individuals in the demonstration for evaluation purposes. This includes information on business activity and on state tax payments. Specifically, we obtained DOR information for three years -- 1990,[59] 1991, and 1992 -- for the following measures: whether the sample member received business income; gross business receipts; whether the business was still open; and sales taxes and business and occupation taxes paid. Similar to the format used for presenting the results based on survey data, below we present the mean values of the outcome measures for the control group, the treatment group, and the calculated unadjusted and regression-adjusted impacts of SEED for each measure.[60]

Impacts on Whether Claimants' Received Business Income. In Table 7.19, we examine the impact of SEED on the likelihood of receiving business income based on DOR

[59]Although the data for 1991 and 1992 are clearly post-enrollment for all demonstration claimants, the measures for 1990 could include business income that occurred before SEED enrollment for some claimants.

[60] The covariates used in these regressions are the same as those used in the models for employment and earnings outcomes based on followup survey data.

records. As shown in the first column, the proportion of control group members who received some business income was relatively stable over the 1990-1992 period. Specifically, in 1990, 13.3 percent of claimants in the control group had businesses that received some income/sales; in 1991, the rate increased slightly to 14.8 percent; and by 1992, the proportion with business income declined slightly to 12.9%.[61] As shown in the second column of Table 7.19, the pattern for treatment group members is somewhat different, with 27.7% of all treatment group members receiving some business income in 1990, and with this rate declining in each successive year to 23.3% in 1991 and 19.1% in 1992.

Table 7.19

Receipt of Business Income

Year	Control	Treatment	Difference	Impact
1990	13.3%	27.7%	14.4%***	15.1%***
1991	14.8%	23.3%	8.5%***	9.4%***
1992	12.9%	19.1%	6.2%***	7.0%***

Consistent with the results based on survey data, both the unadjusted and regression-adjusted differences indicate that claimants in the treatment group are significantly more likely than controls to be active in business during each of the three years, as indicated by having received some business income. In particular, SEED appears to have a very large initial impact on starting a business, but the impact is somewhat less over the longer term. For example, the regression-adjusted impact results indicate that SEED had a 14.4 percentage point increase in the likelihood of having an active business in 1990, which corresponds to over a 100% effect evaluated at the mean of the control group. By 1992, the impact of SEED is estimated to be 7.0

[61] Because of updates in the administrative records, these results are slightly different than the results presented in our interim report (Benus, Johnson, and Wood, 1993).

percentage points, which is considerably lower, but still corresponds to a large percentage impact of over 50% of the control group mean.[62]

Impacts on Whether Business Still Active/Open. In Table 7.20, we provide information on whether the businesses are still open according to DOR records. In the first column, we see that 21.4 percent of all controls had businesses that were still considered to be open at the time the DOR records were collected (July 1993) and nearly three-fourths of these open businesses (15.6 percent) had received some income following random assignment.[63] In contrast, nearly 40 percent of all SEED treatment group members had a business that was still considered to be open, although only about two-thirds (26.6 percent) had received some business income in the three-year period. The estimated impact of SEED on both of these measures is large and statistically significant, indicating that SEED has a considerable impact on the likelihood of having an open and/or active business (70-85 percent effect, evaluated at the control group mean).

Table 7.20

Business Status (July 1993)

Business Status	Control	Treatment	Difference	Impact
Still Open	21.4%	39.3%	17.9%***	19.1%***
Still Open and Received Some Business Income	15.6%	26.6%	11.0%***	12.1%***

[62] In our interim report, we decomposed the SEED businesses into those that were active in the first year only, the second year only, or both the first and second years, to provide information on the timing of business startups and the rate of business failures. These results indicated that roughly one-third of the claimants in both the treatment group and the control group who received business income in the first year did not receive any business income the next year and may have closed. The fact that the business failure rate appears to be similar for treatments and controls is consistent with the results of the hazard rate analyses described earlier in this chapter.

[63] The much larger fraction of controls with open businesses than those who reported positive business income in any of the three years is likely due to individuals who are not active in business not bothering to officially close the business for some time, but rather keep the business license active by simply reporting "no activity" on their state tax return. Moreover, since we obtained the DOR data in July 1993, information on whether the business was still open was based on available data at that time. Because most claimants in our sample were required to report information to DOR only at the end of a year, and were under no obligation to report a business closure until the end of the year (or later) -- six months after we received the data -- it is likely that these data overstate the number of open businesses.

Impacts on Gross Business Income. The Washington State Department of Revenue also maintains records of reported gross business income. This includes gross sales as well as income received for services provided, which although somewhat different than self-employment earnings, is a valuable indicator of the level of activity of businesses established. In Table 7.21, we provide information on gross business income in each of the three years for all SEED participants, regardless of whether the person established a business; thus, individuals who did not start a business during the period (and who are not included in the DOR records) are assigned zero values for their gross business income.

As shown in the first column of Table 7.21, we see that the entire control group averaged $1,935 in gross business income in 1990, $4,734 in 1991, and $3,637 in 1992.[64] A comparison of these amounts with the earnings from self-employment reported in the followup surveys in Table 7.4 indicates that gross business income based on administrative records is much greater. This difference is not evidence that the survey income information were underreported, but rather represents a fundamental difference in what is being measured. Specifically, business income reported in the DOR records include *gross sales, as well as income from services provided*, whereas the survey attempts to measure *earnings* from self-employment, which include deductions for business expenses.

In the second column of Table 7.21, we show that the average gross business income among all treatment group members is considerably larger than for control group members, growing from about $4,135 in 1990 to over $11,000 in both 1991 and 1992. Thus, the estimated impact of SEED on gross business income is substantial and statistically significant in each of the three years. In particular, based on the regression-adjusted results, we estimate the impact of SEED on gross business income to be $2,208 in 1990, $6,836 in 1991, and nearly $8,000 in 1992.[65] Given the mean values of the control group, these impact estimates

[64] If one instead averaged only over those individuals who had positive business income in the year, the average gross income of control group members active in business is about $15,000 in 1990, $32,00 in 1991 and $28,000 in 1992.

[65] It is interesting to note that among those individuals in the treatment group who had positive business income in the year, the average gross income of treatment group members active in business is about $15,000 in 1990, $48,000 in 1991, and $59,000 in 1992. A comparison of these figures with the control group indicates that although the average gross income among those with an active business is similar in 1990, the treatment group members with an active business receive much higher business income in 1991 and 1992 than the controls with an active business in those years.

correspond to increases in gross business income in excess of 100 percent in each of the three years.

Table 7.21

Gross Business Income

Year	Control	Treatment	Difference	Impact
1990	$1,935	$4,135	$2,200**	$2,208***
1991	$4,734	$11,172	$6,438***	$6,836***
1992	$3,637	$11,322	$7,685***	$7,977***

Impacts on Business Tax Payments. An important benefit of small business startups from the State's perspective is the potential tax revenue generated. In Washington State, this includes both sales taxes paid by businesses on products sold, as well as business and occupation (B&O) taxes. In Table 7.22 we provide information on taxes paid by businesses operated by treatment and control group members for the 1990-1992 period.

As this table indicates, over all control group members, the average amount of sales taxes paid in 1990 was $46 and this increased to $121 in 1991 and declined to $100 in 1992. Business and occupation taxes paid by control group members followed the same pattern, but were much lower in each year ($12, $32 and $30, respectively). As shown in the second column, the mean values of taxes paid among all treatment group members is somewhat higher than controls in each of the three years. Moreover, although the differences are not statistically significant for 1990, during which the average level of business activity was relatively low, the differences are statistically significant in both 1991 and 1992. Specifically, the regression-adjusted estimates indicate that SEED increased sales taxes paid by $100 per treatment group member in 1991 and by $214 in 1992. Similarly, the results indicate that SEED increased the business and occupation taxes paid by about $56 per treatment group member in both 1991 and 1992. Consistent with the results described earlier, these impacts on tax payments correspond to roughly 100 percent of the control group mean or larger. Taken together, the results indicate

an increase in total State tax receipts per treatment group member due to SEED of over $150 in 1991 and about $270 in 1992.

Table 7.22

Business Tax Payments

Tax/Year	Control	Treatment	Difference	Impact
Sales Taxes				
1990	$46	$81	$35	$37
1991	$121	$212	$91**	$100**
1992	$100	$304	$204***	$214***
B&O Taxes	$12	$25	$13	$13
1990				
1991	$32	$85	$53***	$56***
1992	$30	$84	$54***	$56***

SEED Impacts on Earnings, Employment and Employees

As described in Chapter 6, in addition to obtaining survey data on wage and salary employment in Washington State, we also obtained UI Wage Records for treatment and control group members who worked in covered employment in the post-program period. In particular, we obtained information on wages and hours worked in covered employment for the quarter the claim was filed and for the following nine calendar quarters. These data were used to construct measures of whether claimants worked in covered employment in the first and second years following enrollment in SEED, hours worked and earnings during the first two years, as well as the composition of total earnings by quarter.[66]

UI Wage Records provide a valuable alternative source of information concerning the earnings of SEED participants. However, because these records are state-based, it is impossible to distinguish individuals who work across the border in a different state -- either in self-employment or in wage and salary employment-- from individuals who do not work at all. This

[66] By the first (second) year following SEED enrollment, we mean the first (second) four complete calendar quarters following the quarter in which the claimant filed for UI benefits and entered the demonstration.

is particularly problematic for claimants in the Vancouver site -- who comprise roughly one-quarter of the SEED analysis sample -- most of whom live within a few minutes of the Oregon border and the Portland metropolitan area. Analysis described in the interim report indicated that the impacts on earnings measures based on UI Wage Records are sensitive to whether Vancouver claimants are included in the analysis. Specifically, we found that the impacts on employment and earnings outcomes are biased towards zero when Vancouver claimants are included. Consequently, the results presented below exclude Vancouver claimants for the employment and earnings analysis based on UI wage records. It should be emphasized, however, that the pattern of the findings and the broad conclusions are not affected by the exclusion of Vancouver claimants from this analysis.

The rest of this section is organized as follows. First, we examine SEED impacts on the likelihood of employment and hours worked in each of the first two years following filing a claim and entering the demonstration. We then examine program impacts on earnings in covered employment using both annual and quarterly measures. The section concludes with a description of the impacts of SEED on job creation (i.e., number of employees) and wages paid to employees based on UI Wage Records.

Impacts on Likelihood of Working in Covered Employment. We begin by examining the impacts of SEED on the likelihood of working in (covered) wage and salary employment during each of the first two years after filing a UI claim and entering the demonstration. This outcome measure is defined to be 1 if UI Wage Records indicated the claimant had positive earnings in any of the first (second) four quarters after filing his/her claim and zero otherwise. In Table 7.23, we report the impact results for these measures of the likelihood of working in (covered) wage and salary employment.

The results in Table 7.23 indicate that 82.6 percent of controls worked in covered employment at some point during the first year after filing the UI claim and entering the demonstration. Among all treatment group members, 78.5 percent worked in covered employment during the same period. The unadjusted and regression-adjusted impacts are similar

and indicate that SEED significantly (at the .05 level) reduced the likelihood of working in covered wage and salary employment by about 4-5 percentage points in Year 1.[67]

The results for the second year follow the same pattern, although the rate of employment for both groups is somewhat lower and the statistical significance of the difference between treatments and controls is slightly weaker. Specifically, we find that 76.6 percent of controls worked in covered wage and salary employment in Year 2, as compared to 72.7 percent of treatment group members, leading to an unadjusted difference of -3.9 percentage points and a regression-adjusted impact of -4.3 percentage points which is significant at the .10 level. Thus, consistent with the results based on followup survey data presented earlier in this chapter, it appears that SEED has a negative and statistically significant impact on the likelihood of working in (covered) wage and salary employment, and that this difference persists through the first two years after entering the demonstration.

Table 7.23

Employment Status

Year	Control	Treatment	Difference	Impact
Worked Year 1	82.6%	78.5%	-4.1%*	-4.7%**
Worked Year 2	76.6%	72.7%	-3.9%	-4.3%*

Impacts on Hours Worked in Covered Employment. To understand the intensity of employment in wage and salary jobs it is important to examine the amount of time worked in such jobs. Washington State is the only State in the U.S. that contains information on hours

[67] Although the SEED demonstration was designed to attract individuals interested in self-employment who were not job-attached, it is likely that some of the claimants who worked in covered wage and salary employment in Year 1 had returned to their same previous employer. To examine this issue, we calculated the percentage of individuals who returned to work to their major employer -- the one they earned the most wages from in the year before filing for UI benefits -- *at some time* during Year 1. Although such a measure does not capture whether the individual directly returned to work at the previous employer after a period of unemployment, it does provide valuable information on whether s/he returned to work with the previous employer at some time within a year. Our results indicate that 25.0 percent of the control group returned to work for their previous employer during Year 1 and that a similar percentage of treatment group members (23.7 percent) also returned to work for their previous employer, a difference which is not statistically significant.

worked in covered employment in its UI Wage Records system. In Table 7.24, we report the impacts of SEED on hours worked in covered employment during the first two years following enrollment in the demonstration.

The findings for hours worked mirror the results for the likelihood of employment and indicate that SEED has a negative impact on working in (covered) wage and salary employment that persists over the observation period. For example, average hours worked in covered employment among all controls in Year 1 was 875. In contrast, treatment group members overall averaged 745 hours in covered wage and salary employment during the first year, resulting in an unadjusted reduction of 130 hours. The regression-adjusted estimate of -138 hours is statistically significant and corresponds to nearly a 16 percentage point reduction in hours worked during the first year.[68]

Table 7.24

Hours Worked in Covered Employment

Year	Control	Treatment	Difference	Impact
Year 1	875	745	-130***	-138***
Year 2	986	895	-91*	-97**

The results for the second post-enrollment year follow the same general pattern, although the level of hours worked increase somewhat. Specifically, during Year 2, controls average 986 hours worked in covered employment, as compared to 895 for treatment group members. This unadjusted difference of -91 hours is significant at the .10 level and the regression-adjusted difference of -97 hours is significant at the .05 level, indicating that the SEED program reduced hours worked in covered employment, and that this effect persisted over the two-year

[68] The difference in hours worked in the first year is in large part due to a difference in work intensity among workers, and not due to the difference in the likelihood of working. For example, among the 82.6 percent of controls who worked at some point during Year 1, average hours worked were 1,060. This compares to 950 hours worked among the 78.5 percent of working treatment group members, and corresponds to a 110 hour reduction in hours worked, or roughly a 10 percent reduction evaluated at the control group mean.

observation period.[69] This is consistent with the impacts reported above for the number of months in wage and salary employment based on the followup survey data.

Impacts on Earnings in Covered Employment. These differences in the likelihood of working in covered employment, coupled with overall reductions in hours worked, suggest potentially important differences between treatments and controls in earnings from (covered) wage and salary employment. In Table 7.25, we report the summary impact results based on UI Wage Records for the first two years after entering the demonstration, as well as detailed results by post-enrollment quarter. As shown in the first row of this table, during the first complete year after filing the UI claim and entering the demonstration, controls earned $10,029 in (covered) wage and salary employment, as compared to only $8,675 for treatment group members.[70] Both the unadjusted reduction of $1,354, and the regression-adjusted impact estimate of -$1,584 are statistically significant and indicate that, consistent with the followup survey results presented earlier, SEED reduced the wage and salary earnings of the treatment group. In fact, the impact estimate for annualized earnings of -$1,477 for Wave I is quite similar to our estimate based on UI Wage Records.

The results for the second complete year after filing the UI claim follow the same general patterns, although the average levels of earnings of both groups are slightly higher and the precision of the estimated program effect is somewhat less. Specifically, during Year 2, controls averaged $12,336 in (covered) wage and salary earnings, as compared to $11,328 for treatment group members. The unadjusted difference estimate of -$1,008 is not statistically significantly different from zero, although the regression-adjusted estimate of -$1,252 is statistically significant at the .10. level. Based on these results, it appears that the reduction in wage and salary earnings due to SEED persists through at least the first two years after enrollment in the program, although there is no evidence that the effect becomes larger over time.

---

[69] In contrast to Year 1, the differences in hours worked among workers are reasonably small in Year 2. Specifically, among claimants who worked in some covered wage and salary employment in Year 2, the average hours worked was 1,287 for controls and 1,231 for treatment group members.

[70] Given that the average earnings of control group members in covered employment in the year before filing for UI benefits was about $21,000, control group members earned less than one-half of their pre-UI annual earnings in the first subsequent year.

In the remainder of Table 7.25 we provide additional evidence on the time pattern of program impacts on wage and salary earnings by decomposing the annual results into separate quarters. As these results indicate, the average earnings in covered employment of control group members increase consistently over the six complete quarters following SEED enrollment, from $1,685 in the first quarter to $3,184 in the sixth quarter, and then remain roughly at the level (or slightly below) over the next three quarters. This pattern is generally expected as control group members return to work in covered employment over time. As shown in Table 7.22, treatment group members follow the same general pattern, although their average earnings are consistently below that of controls in every quarter. Moreover, the regression-adjusted impact estimates indicate that SEED significantly reduced the quarterly earnings of treatment group members by from about $320-470 in each of the four quarters during the first year. The quarterly earnings impacts appear to be somewhat smaller in subsequent quarters (about $250-380) and are estimated with less precision, with only one of the four quarterly impact estimates being statistically significantly different from zero at conventional levels.

Table 7.25

Earnings in Covered Employment

Year/Quarter	Control	Treatment	Difference	Impact
Year 1	$10,029	$8,675	-$1,354**	-$1,584***
Year 2	$12,336	$11,328	-$1,008	-$1,252*
Quarter 1	$1,685	$1,355	-$330**	-$378**
Quarter 2	$2,436	$2,053	-$383**	-$468***
Quarter 3	$2,827	$2,556	-$271*	-$323*
Quarter 4	$3,081	$2,710	-$371**	-$415**
Quarter 5	$3,150	$2,915	-$235	-$300
Quarter 6	$3,184	$2,869	-$315	-$383*
Quarter 7	$3,064	$2,881	-$183	-$248
Quarter 8	$2,938	$2,663	-$275	-$322
Quarter 9	$3,134	$2,922	-$212	-$266

Impacts on Business Employees Using UI Wage Records. Because businesses must submit to the Employment Security Department (ESD) information concerning quarterly wages paid to each employee, it is also possible to utilize UI Wage Records to examine the impacts of SEED on the number of employees hired by demonstration participants. In this way, we could provide an alternative assessment of job creation impacts, which do not rely on interview self-reports.

To address this issue, we first used data to identify businesses owned or operated by SEED treatment or control group members during the post-SEED period. We then obtained from ESD all quarterly wage records available at that time for all individuals who worked for the businesses of any treatment or control group members. At the time the information was obtained, twelve quarters of data from the third quarter of 1990 through the second quarter of 1993 were available.[71] By linking individual worker records for each quarter to a specific business, we could determine the number of different employees in a quarter a business had, as well as total wages paid. For SEED treatment and control group members who were not self-employed, as well as those who were self-employed but who did not submit any records of wages paid, we set the number of employees and wages paid to employees to be zero. Below, we first examine impacts on number of employees.

As shown in Table 7.26, control group members consistently submitted quarterly wage reports for a small number of workers. Although this ranged from .03 employees on average for the third quarter of 1990 to .06 employees during the second quarter of 1992, controls hired .04 employees on average in 8 of the 12 quarters shown in Table 7.26. In contrast, the mean number of employees of the treatment group was .06 during the third quarter of 1990, and then increased reasonably steadily to .22 by the end of this three-year period.

The unadjusted difference estimates and the regression-adjusted estimates are statistically significant in all of the 12 quarters except the first one, and indicate that SEED had a positive impact on job creation. Specifically, over the latest several quarters, we estimate that SEED increased the number of employees by about .17 per treatment group member on average. It

[71] Because data were not readily available for the entire post-program period, it was not possible to create outcome measures that corresponded to the first nine quarters following the quarter of enrollment into SEED. As a result, we decided to analyze the data separately for each calendar quarters.

is important to note that this estimate is not dissimilar to the .25 estimated number of employees per treatment group member based on the Wave II followup survey described earlier. Over the entire sample of 1,507 treatment and control group members, these results suggest that controls as a group employ approximately 30 workers each quarter over all businesses, as compared to about 165 employees working for treatment group members, a net increase of 135 workers.

Table 7.26

Number of Employees

Year/Quarter	Control	Treatment	Difference	Impact
1990, Q3	.03	.08	.05	.05
1990, Q4	.04	.12	.08**	.09**
1991, Q1	.04	.10	.06*	.06*
1991, Q2	.06	.16	.10**	.11**
1991, Q3	.03	.19	.16***	.16***
1991, Q4	.04	.17	.13***	.14***
1992, Q1	.04	.17	.13***	.12***
1992, Q2	.05	.20	.15***	.15***
1992, Q3	.04	.21	.17***	.17***
1992, Q4	.04	.20	.16***	.16***
1993, Q1	.04	.21	.17***	.16***
1993, Q2	.04	.22	.18***	.17***

Impacts on Wages Paid to Employees Using UI Wage Records. We also used UI Wage Records to examine the impacts of SEED on the total wages paid to the employees of businesses operated by treatment and control group members. The results by calendar quarter are shown in Table 7.27. Consistent with the results for number of employees, for the most part, these results indicate a generally increasing pattern of total wages paid to employees for controls and a much stronger increasing pattern for treatment group members. For example, in the third quarter of 1990, at the time when the last claimants were being enrolled in SEED,

control businesses on average paid $12 in employee wages, as compared to $82 for treatment group businesses. The unadjusted difference of $70 is statistically significant at the .05 level as is the regression-adjusted estimate of $73.

By two to three years later, the employee wage bill for control businesses had increased on average to about $75-100 per quarter, as compared to $475-500 per quarter for treatment group businesses. This resulted in difference estimates and regression-adjusted impact estimates of approximately $375-400 per quarter increase in the total wages paid to employees due to SEED. Over the entire three-year observation period of these data, these results indicate that SEED increased total wages paid to employees by about $2.5 million.

Table 7.27

Total Employee Wages Paid

Year/Quarter	Control	Treatment	Difference	Impact
1990, Q3	$12	$82	$70**	$73**
1990, Q4	$22	$194	172***	$182***
1991, Q1	$43	$195	$152**	$159**
1991, Q2	$89	$274	$185**	$195**
1991, Q3	$54	$328	$274***	$284***
1991, Q4	$65	$357	$292***	$293***
1992, Q1	$56	$379	$323***	$324***
1992, Q2	$70	$426	$356***	$352***
1992, Q3	$73	$475	$402***	$402***
1992, Q4	$85	$492	$407***	$402***
1993, Q1	$108	$509	$401***	$397***
1993, Q2	$104	$486	$382***	$370***

IMPACTS OF SEED FOR VARIOUS CLAIMANT SUBGROUPS

We also examined whether the impacts of SEED on the self-employment and wage and salary employment outcome measures varied by numerous claimant characteristics, including gender, race, site, and whether the individual intended to return to the previous employer at the time of application to SEED. To test for differential program impacts across subgroups, we added interaction terms (e.g., treatment status interacted with subgroup characteristic(s)) to the basic multivariate regression model described earlier in this chapter. In this section we describe the general patterns of the subgroup results for the main outcome measures derived from the followup survey data that were statistically significant in both Wave I and Wave II; readers interested in additional details can refer to Appendix C.

First, we describe the patterns of subgroup impact differences for the self-employment outcome measures. The results of these analyses indicate that the SEED Demonstration had a significantly greater impact on females than on males in increasing the likelihood of self-employment.[72] Specifically, the regression-adjusted impact estimate indicates that the program had a 16-17 percentage point greater impact in increasing the likelihood of self-employment for women than men. However, there were no significant differences in the impact of SEED by gender on the number of months in self-employment or on the likelihood of being self-employed at either followup survey.[73] This suggests that the rate of self-employment terminations are higher among women than men.[74]

---

[72] It is not clear why the program had a greater impact on women's participation in self-employment over the observation period than on men's participation. One possible explanation is that women are more receptive to the types of services provided by the SEED program. An alternative explanation is that the program may have provided superior assistance for the types of enterprises that women were most interested in. A third explanation may be that women have a more difficult time in obtaining financing and that the SEED lump-sum payment may have been relatively more helpful to women in overcoming this disadvantage.

[73] The Wave I results indicated that SEED had a larger impact in increasing time in self-employment for females than males, but this effect was much reduced and not statistically significant for the entire 33-month observation period.

[74] The results of the hazard rate analysis of the time until transition from the first self-employment spell is consistent with this speculation. Specifically, among those who started a self-employment spell, the estimated hazard of terminating the self-employment spell is higher for male controls than male treatment group members, whereas the estimated hazards are consistently higher for female treatment group members than for female controls. However, these differences are not significant at conventional levels, in part due to the very small sample sizes involved.

In contrast to the results for self-employment outcome measures, our results indicate that the effects of SEED on wage and salary outcome measures are widespread and not concentrated in specific subgroups. Specifically, none of the interaction terms were significant for both Wave I and II.[75]

The final set of subgroup results concern the outcome measures for combined self-employment and wage and salary employment. For the likelihood of being employed at some time during the observation period, there are two consistent patterns of interaction effects for both Wave I and II. In particular, the results indicate that the impact of SEED on the likelihood of being employed in some type of job was greater for whites than for nonwhites.

There are also strong differences in the impacts of SEED on months worked and on total combined earnings by business ownership status at application. For example, using the Wave I data, the impact of SEED on annualized time worked for those who owned a business at the time of application was 4.1 months per year, as compared to 1.9 months per year for those who previously owned a business (but not at the time of application), and as compared to .6 months per year for those who had never been self-employed. Moreover, it seems that the impact of SEED on total earnings is positive and quite large for those who were business owners at application, and the impact is relatively small and negative for those who were non-owners at application. The results for the entire 33-month observation period follow the identical pattern.

SUMMARY

In this chapter we presented our detailed results concerning the impacts of SEED on numerous self-employment and wage and salary employment outcome measures, as well as on job creation and UI benefits receipt. In this section, we briefly summarize the highlights of these findings. In particular, we focus on the main self-employment and wage and salary

[75] For Wave I, our results indicated that there were differences in the impact of SEED on the likelihood of wage and salary employment and on months in wage and salary employment between those in the top quartile of the prior earnings distribution than for those in the lowest quartile. In particular, the impact of SEED was to reduce the likelihood of wage and salary employment and number of months worked for those in the top quartile by a greater amount than those in the bottom quartile. Although the pattern was similar for Wave II, the interaction effect was not statistically significant at conventional levels.

employment and earnings measures based on the followup surveys and on the receipt of UI benefits that contribute to the benefit-cost results described in Chapter 10.

The main impact results of the SEED program tested in Washington State are summarized in Table 7.28. As these results indicate, the SEED program had a very large and positive impact on the self-employment experiences of UI claimants that persisted throughout both the Wave I and Wave II survey. Specifically, treatment group members are estimated to be much more likely than controls to have a self-employment experience (by about 22-25 percentage points); to spend more time per year in self-employment (by 2.0-2.3 months); and are more likely to be still self-employed at the time of the followup surveys (by 12-16 percentage points). Consistent with these results, we find that the SEED program increased claimants' self-employment earnings by over $1,600 per year. These estimated impacts are quite large and correspond to effects that are roughly from 50 percent to over 100 percent of the control group mean.

In contrast to the estimated impacts on self-employment outcomes, the impacts on wage and salary employment measures are consistently negative and indicate that SEED significantly reduced these indicators of wage and salary employment in both the short- and long-term. Specifically, SEED reduced claimants' likelihood of working in wage and salary employment by 6 percentage points, reduced number of months per year working in wage and salary employment and reduced earnings from wage and salary employment by from about $1,400-$1,800 per year. These effects, while statistically significant and economically important, represent smaller percentage changes. Specifically, these impacts correspond roughly to 10-20 percent effects evaluated at the mean of the control group.

In the bottom panel of Table 7.28 we present the impacts of SEED on total employment and earnings outcomes. These combined results do not follow as strong or as consistent of a pattern as the separate results for both self-employment and wage and salary employment measures. The first three rows taken together suggest that the SEED program was fairly successful in assisting claimants become reemployed. Specifically, we find that the program significantly increased the likelihood of claimants' finding employment at some time in Wave I, increased the number of months worked per year (by 1.1-1.3 months per year), and slightly increased the likelihood of being employed at the time of the Wave II survey. On the other

Table 7.28
Washington Summary
Program Impacts

	Wave I	Wave II
Self-Employment		
Percent Self-Employed Since Random Assignment	+25%***	+22%***
Time Self-Employed Since Random Assignment	+2.3*** months	+2.0*** months
Percent Self-Employed at the End of Observation Period	+16%***	+12%***
Earnings from Self-Employment Since Random Assignment	+$1,596**	+$1,675**
Wage and Salary Employment		
Percent Employed in W&S Since Random Assignment	-7%***	-6%***
Time Employed in W&S Since Random Assignment	-0.6** months	-0.7** months
Percent Employed in W&S at the End of Observation Period	-7%**	-3%*
Earnings from W&S Since Random Assignment	-$1,407*	-$1,780**
Total Employment		
Percent Employed Since Random Assignment	+4%***	+0%
Time in Employment Since Random Assignment	+1.3*** months	+1.1*** months
Percent Employed at the End of Observation Period	+3%	+6%**
Total Earnings Since Random Assignment	+$566	+$289

hand, the impacts on total earnings are insignificantly different from zero for both Wave I and II. Thus, it appears that SEED did not increase claimants' total earnings from all types of employment.

We also examined in detail the impacts of SEED on total benefits receipt. Although not reported in this table, our results indicate that after taking into account the lump-sum payment provided to treatment group members who met the required milestones for starting a business, SEED increased total benefit payments by $1,098 during the initial benefit year. Because the estimated impact of SEED on subsequent receipt of UI was small and negative, our results indicate that SEED increased total benefits by $1,013 per claimant over the entire observation period.

8

MASSACHUSETTS DEMONSTRATION IMPACTS

In this chapter we present the impacts of the Massachusetts Enterprise Project. The results are presented in two sections. In the first section, we present our main impact results based on an evaluation of Cohorts 1 and 2. In the second section of this chapter, we evaluate Cohort 3 separately. As discussed in Chapter 4, exogenous developments in Massachusetts altered the implementation of the demonstration, especially during the period when Cohort 3 was enrolled. As a result, we concluded that it is not appropriate to combine all three cohorts for the impact analysis; instead, we combine only Cohorts 1 and 2 for the main impact analysis. We present the Cohort 3 impact results largely for completeness. Further analysis is required to determine if and how all three cohorts can be pooled to obtain an overall impact estimate.

As we did in the evaluation of the SEED Demonstration impacts, we measure program impacts by comparing the experiences of treatment group members with the experiences of control group members following random assignment. Within each of the two sections in this chapter, we follow the same presentation pattern as was used earlier for the SEED evaluation. First, we describe the demonstration impacts on self-employment outcomes, including the likelihood of becoming self-employed, total time in self-employment, the likelihood of being self-employed at the time of the followup surveys, and total earnings from self-employment. We then examine the impact on similar measures for wage and salary employment. Next, we combine self-employment with wage and salary employment outcomes and examine the demonstration impacts on total employment and earnings. Following this evaluation of employment and earnings impacts, we present the demonstration impacts on such outcomes as job creation, length of UI spells, and UI benefits receipt. Throughout the chapter, we also provide information about the impact of the demonstration on key claimant subgroups.

As in Chapter 7, we present results for two overlapping time periods. First, we present impacts based on data obtained from the Wave I survey, which, in Massachusetts, was administered on average 19 months after random assignment. Second, we present impacts based on a combination of Wave I and Wave II survey data. These data are used to create a 31-month (on average) observation period, beginning with random assignment and ending with the Wave II survey. In the tables below, impact estimates derived from the 19-month observation period are referred to as *Wave I* results, while impact estimates derived from the 31-month observation period are referred to as *Wave II* results.

Impacts Based on Cohorts 1 and 2

In this section, we present impact estimates based on the combined samples from Cohorts 1 and 2. Combining these two cohorts yields a total of 263 treatment group members and 258 control group members. The 86 percent Wave I survey response rate yields a total sample of 449 for the Wave I impact analysis (229 treatment group members and 220 controls). The 89 percent Wave II response rate yields a total sample of 398 for the Wave II impact analysis (204 treatment group members and 194 controls).

Self-Employment Impacts

This subsection covers the impacts of the Enterprise Project on self-employment outcomes. The next subsection examines the demonstration impacts on wage and salary employment outcomes. In the final subsection, we combine the self-employment with wage and salary employment outcomes and evaluate demonstration impacts on combined employment and earnings outcomes.

Impacts on Likelihood of Self-Employment Experience. As in the SEED Demonstration, the Enterprise Project provided business training, counseling, and financial assistance to targeted UI claimants who were interested enough in self-employment to both attend an information session and submit an application, and who were randomly selected into the treatment group. To the extent that the demonstration services provided effective assistance to

treatment group members, one would expect more treatment group members than control group members to enter self-employment.

The results in Table 8.1 indicate that, indeed, treatment group members were much more likely than control group members to enter self-employment. Specifically, within the first 19 months after random assignment (Wave I), 52 percent of the treatment group, as compared with 35 percent of the control group, were self-employed at some point (i.e, had at least one self-employment spell).[76] The 17 percentage point difference in the likelihood of self-employment for the two groups is statistically significant (at the .01 level) and corresponds to about 50 percent of the control group mean.

Table 8.1

Self-Employment Experience Since Random Assignment

Massachusetts, Cohorts 1 and 2

	Control	Treatment	Difference	Impact
Wave I	35%	52%	+17%***	+17%***
Wave II	47%	58%	+11%**	+12%**

As indicated in Table 8.1, additional control and treatment group members entered self-employment between the two surveys. That is, for the control group, the likelihood of having a self-employment spell increased from 35 percent in Wave I to 47 percent in Wave II. For the treatment group, the self-employment likelihood increased from 52 to 58 percent. As a result of the greater influx by control group members into self-employment after the Wave I survey, the simple difference in the likelihood of having a self-employment experience between the treatment and control groups declined from 17 percentage points in Wave I to 11 percentage

[76] The Wave I results reported here differ slightly from the Wave I findings presented in our earlier report (Benus, Wood and Grover, 1994), although all of the main findings are qualitatively similar. The differences are due to minor definitional changes in the self-employment experience variable.

points in Wave II. The difference in Wave II corresponds to less than 25 percent of the control group mean. This difference remained statistically significant (at the .05 level).

The regression-adjusted impact estimates of the likelihood of having a self-employment experience, reported in the last column of Table 8.1, are quite similar to the simple-difference estimates reported in the third column.[77] Based on these regression-adjusted impacts of 17 percentage points in Wave I and 12 percentage points in Wave II, we conclude that the Enterprise Project had a large positive impact on the likelihood of becoming self-employed.

The impact results reported in the last column of Table 8.1 are based on logit regression models that control for a number of covariates. Although not reported in the table, the regression results indicate that several of the covariates significantly affect the likelihood of entering self-employment in both the Wave I and Wave II.[78]

Impacts on Time in Self-Employment. In this section we evaluate the impact of the Enterprise Project on the time spent in self-employment after random assignment. This analysis, uses annualized measures of the number of months in self-employment following random assignment. Specifically, using information on start dates and end dates of self-employment spells from the followup surveys, annualized time in self-employment is calculated as the proportion of the period from random assignment to the interview date that an individual sample member was self-employed multiplied by 12.

As shown in Table 8.2 and consistent with the results described above, treatment group members spent more time in self-employment on average than controls. On an annualized basis, treatment group members spent 2.5 months per year in self-employment during the first 19 months after random assignment (Wave I) and slightly more (2.6 months per year) during the entire 31-month observation period (Wave II). Treatment group members, on average, spent approximately one month more per year in self-employment than control group members.

---

[77] The covariates used in all the Massachusetts regressions are presented and defined in Appendix A.

[78] In Appendix B, we identify all the significant covariate coefficients, as well as their signs, for each of the regressions presented. In this regression, for example, we find that in both the Wave I and Wave II regression models males are more likely to enter self-employment, while those who previously worked in the service industry are less likely to enter self-employment.

Similar results are obtained from the regression-adjusted impacts.[79] Based on these results, we estimate that treatment group members on average worked approximately 2.2 months more in self-employment than controls during the 31-month observation period.[80]

Table 8.2

Time in Self-Employment Since Random Assignment

Massachusetts, Cohorts 1 and 2

	Control	Treatment	Difference	Impact
Wave I	1.4 months	2.5 months	+1.1*** months	+1.0*** months
Wave II	1.7 months	2.6 months	+0.9** months	+0.8* months

Impacts on Self-Employment at Time of the Followup Surveys. The results described above indicate that treatment group members were more likely than control group members to have a self-employment spell during the observation period, and that they spent more time self-employed than controls. Although these results confirm that the Enterprise Project was successful in encouraging business startups, they do not necessarily indicate program success. A better indicator of program success is the proportion of individuals remaining in self-employment at the time of the followup surveys.

In Table 8.3, we present information on the proportion of treatment and control group members who were self-employed at the time of each followup survey. As this table indicates, 40 percent of the treatment group was self-employed at the time of the Wave I survey, as compared to 29 percent of the control group. This corresponds to a 11 percentage point difference, which is about a 38 percent effect at the control group mean (statistically significant at the .05 level). By the time of the Wave II survey, the number of controls in self-employment

[79] Only one covariate was statistically significant in both the Wave I and Wave II regression models: being in the service industry. Those who previously worked in the service industry spent less time in self-employment after random assignment than those who worked in other industries.

[80] This estimate is derived by multiplying the Wave II impact estimate (0.8 months) by 31/12.

had increased by 9 percentage points to 38 percent, while the proportion of treatment group members in self-employment had only increased slightly (by 3 percentage points) to 43 percent. At the time of the Wave II survey, therefore, the simple-difference estimator declined to 5 percentage points, which corresponds to only a 13 percent effect of the control group mean and is statistically insignificant. As indicated in the table, the regression-adjusted logit impacts (presented in the last column) are similar to the simple-difference estimates (column 3).[81]

Table 8.3

Self-Employment at Two Different Observation Times

Massachusetts, Cohorts 1 and 2

	Control	Treatment	Difference	Impact
Wave I	29%	40%	+11%**	+11%**
Wave II	38%	43%	+5%	+5%

Impacts on Self-Employment Earnings. In Table 8.4, we present estimates of program impact on self-employment earnings. Similar to the measure of time in self-employment, earnings from self-employment is constructed as an annualized measure. Moreover, the earnings figures have been deflated to 1990 dollars using a CPI index. As shown in Table 8.4, during the Wave I observation period, annualized self-employment earnings for the control group averaged $1,404 in 1990 dollars.[82] In contrast, annualized self-employment earnings during Wave I for the treatment group averaged $2,916. Neither the unadjusted impact of $1,512, nor the regression-adjusted impact of $1,218, are significant at the .10 level.[83]

[81] There were no covariates that were statistically significant in both the Wave I and II regression models for the likelihood of being self-employed at the time of the followup surveys.

[82] This average earnings figure includes cases where self-employment earnings were reported to be zero. To the extent that some of the zero responses are likely to be misreported, the above average earnings figure is likely to understate actual self-employment earnings.

[83] The only covariate that significantly affected self-employment earnings in both the Wave I and Wave II models was prior employment in the service industry (-).

The findings for Wave II are quite similar to the Wave I results. The unadjusted annualized impact estimate is $1,188 and the regression-adjusted impact estimate is $1,219. Neither of these impact estimates is significant. Thus, we conclude that the Enterprise Project did not significantly affect self-employment earnings.

Table 8.4

Earnings From Self-Employment Since Random Assignment

Massachusetts, Cohorts 1 and 2

	Control	Treatment	Difference	Impact
Wave I	$1,404	$2,916	+$1,512	+$1,218
Wave II	$1,439	$2,627	+$1,188	+$1,219

In summary, the impacts of the Massachusetts demonstration on self-employment outcomes are inconsistent. The Enterprise Project had significant positive impacts on some of the self-employment outcomes analyzed, while on others the effect was insignificant. For example, in both observation periods, treatment group members were more likely than controls to have a self-employment experience and to spend more time per year in self-employment. In contrast to the above significant impacts, the demonstration did not have a statistically significant impact on self-employment earnings during either of the two observation periods.

Wage and Salary Employment Impacts

In this section we continue our focus on the employment and earnings impacts of the Massachusetts demonstration, but shift our attention to examine the impacts of the demonstration on the wage and salary employment experiences of program participants.

Impacts on Likelihood of Wage and Salary Employment Experience. In Table 8.5 we examine the impacts of the Enterprise Project on the likelihood of having a wage and salary employment experience during the observation period. These results indicate that approximately the same proportion of the treatment group as the control group had at least one wage and salary

job during the observation period. During the Wave I observation period, 64 percent of the control group and 65 percent of treatment group had at least one wage and salary job; during the Wave II observation period, 75 percent of control group members and 71 percent of treatment group members had at least one wage and salary job. Neither the Wave I nor the Wave II treatment/control group difference is statistically significant, indicating that the Enterprise Project did not have an impact on the likelihood of wage and salary employment.

Table 8.5

Wage and Salary Employment Experience Since Random Assignment

Massachusetts, Cohorts 1 and 2

	Control	Treatment	Difference	Impact
Wave I	64%	65%	+1%	+1%
Wave II	75%	71%	-4%	-4%

In the last column of Table 8.5 we present the impact results derived from logit regression models for the probability of having a wage and salary job during the observation period. These results confirm our conclusion that the Enterprise Project did not affect the likelihood of having a wage and salary job during either the Wave I or the Wave II observation period.[84]

Impacts on Time in Wage and Salary Jobs. In Table 8.6, we report the impacts of the Enterprise Project on annualized months in wage and salary jobs during the two observation periods. During the first 19 months after random assignment (Wave I), treatment group members worked 3.8 months per year in wage and salary jobs compared to 3.2 months per year for controls. The increase of 0.6 months per year was statistically insignificant at the .10 level.

[84] As reported in Appendix B, holding other variables constant, three factors have a statistically significant impact on the likelihood of having a wage and salary job in both observation periods. These factors are: prior employment in the service industry (+), being a participant in the Milford site (+), and having a benefit start date in the first quarter of 1991 (+).

The results for the entire 31-month observation period (Wave II) indicate that both the treatment and control groups increased their annual time spent in wage and salary jobs. The difference between the two groups in annual time spent in wage and salary employment, however, remained small and insignificant (0.3 months).

Table 8.6

Time in Wage and Salary Employment Since Random Assignment

Massachusetts, Cohorts 1 and 2

	Control	Treatment	Difference	Impact
Wave I	3.2 months	3.8 months	+0.6 months	+0.8** months
Wave II	4.1 months	4.4 months	+0.3 months	+0.6 months

The results for the entire 31-month observation period (Wave II) indicate that both the treatment and control groups increased their annual time spent in wage and salary jobs. The difference between the two groups in annual time spent in wage and salary employment, however, remained small and insignificant (0.3 months).

The regression-adjusted impacts shown in the last column of Table 8.6 are somewhat higher than the simple-difference estimates. Moreover, the Wave I regression-adjusted impact estimate is statistically significant at the .05 level. Thus, during Wave I, the Enterprise Project increased the time spent in wage and salary jobs.[85]

Impacts on Wage and Salary Employment at Time of the Followup Surveys. In Table 8.7, we provide information on the proportion of sample members who were employed

[85] Covariates that were statistically significant in both the Wave I and Wave II regression models for time in wage and salary employment include whether the claimant was male (-), whether the claimant was previously employed in the service industry (+), and whether UI benefits started during the first quarter of 1991 (+). Specifically, the results indicate that holding other factors constant, males spent less time in wage and salary employment than females, that the time spent in wage and salary employment was higher for those who were previously employed in the service industry, and that claimants whose benefits started in the first quarter of 1991 spent more time in wage and salary employment than those whose benefits started in other periods.

in wage and salary jobs at the time of the two followup surveys. As this table indicates, at the Wave I followup survey, 46 percent of control group members and 52 percent of treatment group members were employed in wage and salary jobs. This difference of 6 percentage points matches the regression-adjusted logit estimate of 6 percentage points. This Wave I estimate is statistically significant (at the .10 level).

Table 8.7

Wage and Salary Employment at Two Different Observation Times

Massachusetts, Cohorts 1 and 2

	Control	Treatment	Difference	Impact
Wave I	46%	52%	+6%	+6%*
Wave II	54%	56%	+2%	+1%

As indicated in the table, the difference between treatment and control groups in the likelihood of wage and salary employment is lower at the time of the Wave II survey. Specifically, at the time of the Wave II survey, 56 percent of treatment group members were employed in wage and salary jobs, as compared to 54 percent of controls. The small 2 percentage point difference is not statistically significant; the regression-adjusted impact is also insignificant.

Impacts on Wage and Salary Earnings. The results for annualized earnings in wage and salary employment are shown in Table 8.8. Consistent with our measure of self-employment earnings, earnings in wage and salary employment are expressed in annualized terms and measured in constant 1990 dollars. These results indicate that during the initial 19-month observation period, treatment group members earned $9,311 per year on average as compared to $6,613 per year for control group members.[86] This yields an unadjusted impact

[86] This average annualized earnings figure includes some cases where wage and salary earnings were reported to be zero. To the extent that some of the zero responses are likely to be misreported, the above average earnings figure is likely to understate actual wage and salary earnings. However, this data problem is much less serious than

of +$2,698, which is significant at the .05 level. After controlling for other factors, the regression-adjusted impact is estimated to be +$3,230, also significant at the .05 level.

Table 8.8

Earnings From Wage and Salary Employment Since Random Assignment

Massachusetts, Cohorts 1 and 2

	Control	Treatment	Difference	Impact
Wave I	$6,613	$9,311	+$2,698**	+$3,230**
Wave II	$7,797	$10,119	+$2,322*	+$3,053**

The results for the entire 31-month observation period are similar. As described in Table 8.8, average annualized earnings in wage and salary employment for the treatment group over the entire observation period was $10,119, as compared to $7,797 for the control group. This unadjusted difference of +$2,322 is statistically significant at the .10 level. Moreover, after controlling for other factors, the regression-adjusted annualized impact on wage and salary earnings is +$3,053, which is statistically significant at the .05 level. Thus, we conclude that the Enterprise Project had a positive and significant impact on the annual earnings of participants from wage and salary jobs during the observation period.[87]

Total Employment and Earnings Impacts

The results described above indicate that the Enterprise Project had positive and statistically significant impacts on the probability and duration of self-employment, but not on self-employment earnings. The results also indicate positive and significant impacts on wage and salary employment outcomes (except on the likelihood of wage and salary employment).

---

for self-employment earnings.

[87] The only covariates that were statistically significant in predicting wage and salary earnings for both Wave I and Wave II were whether the claimant was previously employed in the service industry (+) and whether UI benefits started in the first two quarters of 1991 (+).

In this section, we present our estimates of the impact of the Enterprise Project on combined self-employment and wage and salary employment outcomes.

Impacts on Likelihood of Employment. In the analysis presented above, we found that the Enterprise Project had a positive and significant impact, during both observation periods, on the likelihood of having at least one self-employment experience. Moreover, we found that the impact of the demonstration on the likelihood of having at least one wage and salary job during both observation periods was insignificant. In Table 8.9, we combine the two types of employment (self-employment and wage and salary employment) and examine the impacts of the demonstration on the likelihood of having at least one employment experience -- either self-employment or a wage and salary job. These results indicate that during the initial 19-month observation period, the Enterprise Project had a significant and positive impact on the likelihood of employment. Specifically, 93 percent of the treatment group was employed in either wage and salary or self-employment during this period, as compared to 82 percent of controls. The unadjusted 11 percentage point difference is statistically significant at the .01 level. The regression-adjusted impact estimate of 11 percentage points is also significant at the .01 level.

Table 8.9
Combined Employment Experience Since Random Assignment
Massachusetts, Cohorts 1 and 2

	Control	Treatment	Difference	Impact
Wave I	82%	93%	+11%***	+11%***
Wave II	92%	97%	+5%**	+5%**

Based on these results, it is clear that the Enterprise Project significantly facilitated the reemployment of program participants.

Over the entire 31-month observation period (Wave II), this positive impact persisted. Specifically, during the Wave II observation period, 97 percent of the treatment group and 92 percent of the control group held a job or were self-employed at some point. The 5 percentage point impact, while smaller than the 9 percentage point impact in Wave I, remains statistically significant (at the .05 level). Thus, the Enterprise Project not only facilitated reemployment of

program participants, but also positively affected the likelihood of employment well into the future.

Impacts on Time in Either Wage and Salary Employment or Self-Employment. The results described earlier indicated that the Enterprise Project significantly increased the amount of time spent in self-employment during both observation periods. The results also showed that, during Wave I, the Enterprise Project also increased the time in wage and salary jobs. To examine the overall effects of the Enterprise Project on time in either self-employment or wage and salary employment, we created an annualized measure of combined months employed in either type of employment. The total measure is not simply the sum of the separate annualized measures of time in self-employment and time in wage and salary employment.[88] Rather, the combined measure takes into account information on the dates of each employment experience to avoid double-counting periods during which individuals are working at both self-employment and wage and salary employment.

Annualized total time in either self-employment or wage and salary employment is presented in Table 8.10. For the entire control group, the average annual time in either type of employment during the initial 19-month observation period was 4.5 months. This compares to 6.5 months per year for the treatment group. The unadjusted impact of 2.0 months is statistically significant at the .01 level, as is the regression-adjusted impact estimate of 2.1 months.

Table 8.10

Total Time in Employment Since Random Assignment

Massachusetts, Cohorts 1 and 2

	Control	Treatment	Difference	Impact
Wave I	4.5 months	6.5 months	+2.0*** months	+2.1*** months
Wave II	5.8 months	7.4 months	+1.6*** months	+1.9*** months

[88] As a result, the overall impacts of the Enterprise Project on this measure do not equal the simple sum of the impacts on time in self-employment and time in wage and salary employment. Moreover, the combined results are not equal to the sum of the separate impacts because of differences in sample size due to missing data on one or the other components.

The results are somewhat higher for the entire 31-month observation period. The annualized time in employment increased for both the treatment and control groups -- to 7.4 months and 5.8 months, respectively -- resulting in a simple-difference estimate of 1.6 months per year. The corresponding regression-adjusted estimate was 1.9 months per year, which, like the simple-difference estimator, was also statistically significant at the .01 level. Thus, we conclude that the Enterprise Project unambiguously increased total time in employment by approximately two months per year, or nearly 6 months over the entire observation period.

Impacts on Employment at Time of the Followup Surveys. The results of the previous sections indicate that the Enterprise Project significantly increased the likelihood of being self-employed at the time of the Wave I survey, but not at the time of the Wave II survey. Similarly, previous results indicated that employment in wage and salary jobs was also positively affected by the demonstration at the time of the Wave I survey, but not at the time of the wave II survey. In Table 8.11, we examine the impact of the Enterprise Project on the likelihood of being employed in either type of employment at the time of the two surveys.

Table 8.11
Employment at Two Different Observation Times
Massachusetts, Cohorts 1 and 2

	Control	Treatment	Difference	Impact
Wave I	68%	81%	+13%***	+13%***
Wave II	81%	87%	+6%	+6%*

As indicated in Table 8.11, 68 percent of the control group and 81 percent of the treatment group were employed in either self-employment or a wage and salary job at the time of the Wave I survey. The simple difference of 13 percentage points as well as the regression-adjusted impact of 13 percentage points, are both statistically significant at the .01 level.

At the time of the Wave II survey, 87 percent of treatment group members and 81 percent of controls were employed in either self-employment or wage and salary employment.

The simple difference of 6 percentage points was not statistically significant, but the 6 percent regression-adjusted impact was statistically significant at the .10 level.

Impacts on Total Earnings. The final results in this section focus on total earnings from both wage and salary jobs and self-employment. As shown in Table 8.12, average annualized earnings from both wage and salary employment and self-employment over the 19-month observation period for the control group was $8,483. Total annualized earnings over the same period for the treatment group averaged $13,151. The unadjusted annualized impact of $4,668 and the regression-adjusted impact of $4,764 are both significant at the .05 level.

Table 8.12
Combined Earnings From Employment Since Random Assignment
Massachusetts, Cohorts 1 and 2

	Control	Treatment	Difference	Impact
Wave I	$8,483	$13;151	+$4,668**	+$4,764**
Wave II	$10,056	$14,664	+$4,608**	+$5,940***

The results for the entire 31-month observation period are slightly higher. The average annual rates of earnings over this period were $10,056 for controls and $14,664 for the treatment group. The simple-difference estimate of $4,608 per year is similar to the Wave I impact estimate of $4,764. The regression-adjusted Wave II impact estimate of $5,940 is substantially higher than the Wave I impact estimate. Thus, we can firmly conclude that the Enterprise Project had a significant and positive impact on combined annual earnings from self-employment and wage and salary employment.

Other Demonstration Impacts

Impacts on Job Creation. In addition to providing employment for the business owner, small businesses often generate wage and salary employment for others. This section analyzes the demonstrations' impacts on the wage and salary employment of nonparticipants by measuring the total employment in businesses operated by treatment group members and compare this total with total employment in businesses operated by control group members. The difference

between the total employment in treatment group businesses and total employment in control group businesses represents an estimate of the demonstration's impacts on the employment of nonparticipants.

When small business owners need employees they sometime hire family members. Small businesses also generate employment for nonfamily members. In previous reports we examined the employment of family and nonfamily members separately (see Benus, Wood, and Grover, 1994). Here, however, we analyze the demonstration impacts on total nonparticipant employment. Nonparticipant employment includes all family and nonfamily wage and salary employees of the business, excluding the demonstration participant him/herself.

We measure the effect of the demonstration on nonparticipant employment as the difference between the mean number of employees per treatment group member and the mean number of employees per control group member. Since this analysis is conducted over all treatment and control group members (whether they have a business or not), those individuals without a business are assumed to have no employees.

As indicated in Table 8.13, the Enterprise Project did not significantly increase the employment of nonparticipants. Indeed during both Wave I and Wave II, the mean number of employees per control group member was higher than the mean for treatment group members. A closer examination of this result indicates that the high mean for the control group is dramatically influenced by a few large businesses (in terms of employment). Specifically, we found that a few control group businesses had a large number of employees (over 20 employees each). We did not find similar sized businesses among the treatment group.

Table 8.13
Job Creation Impacts per Members: Total Employment
Massachusetts, Cohorts 1 and 2

	Control	Treatment	Difference	Impact
Wave I	0.7	0.3	-0.4	-0.4
Wave II	1.7	0.8	-0.9	-1.0

Impacts on Length of UI Spell. Using data from the UI history files, we developed a measure of the number of weeks in the first spell of UI benefits receipt. As shown in Table 8.14, on average, controls experienced a first spell of UI benefits of 26.5 weeks while treatment group members experienced a 24.5 week first spell. Thus, the impact of the Massachusetts Demonstration was to reduce the length of the first UI spell by approximately two weeks.

Table 8.14

Length of First UI Spell

Massachusetts, Cohorts 1 and 2

Control	Treatment	Difference	Impact
26.5 weeks	24.5 weeks	-2.0*** weeks	-1.8*** weeks

Impacts on UI Benefits. Table 8.15 presents the program impacts on UI benefits received in each year of the observation period. In Year 1, control group members received an average of $6,845 in UI benefits while treatment group members received an average of $6,120 (for a simple difference of -$725). The regression-adjusted impact of the demonstration on UI receipt in the first year was -$768 (statistically significant at the .01 level), indicating that the Massachusetts Demonstration lowered UI benefits receipt during the first year. In subsequent years, the demonstration did not have a significant impact on UI benefits receipt. In Year 2, control group members received an average of $410 in UI benefits, while treatment group members received an average of $307 (a difference of -$103). During the first seven months of Year 3 (i.e., months 25 to 31 after the initial UI claim), treatment group members averaged $141 in UI benefits and control group members averaged a nearly identical $140. Over the entire 31-month observation period, the demonstration's total impact on UI benefits was -$876 (statistically significant at the .01 level).

Table 8.15

UI Benefit Amounts

Massachusetts, Cohorts 1 and 2

Time Frame	Control	Treatment	Difference	Impact
Year 1 (Months 1-12)	$6,845	$6,120	-$725***	-$768***
Year 2 (Months 13-24)	$410	$307	-$103	-$89
Year 3 (Months 25-31)	$141	$140	-$1	-$13
Total Wave II (Months 1-31)	$7,400	$6,567	-$833***	-$876***

Impacts Based on Cohort 3

In Massachusetts, changes in UI policy altered the implementation of the demonstration, especially for Cohort 3.[89] As a result, Cohorts 1 and 2 were combined for the main impact analysis. Cohort 3 impact results are presented below largely for completeness. Future analysis may shed light on how all three cohorts might be pooled to obtain an overall impact estimate for the entire Massachusetts sample.

As described in Chapter 4, in Massachusetts, a total of 701 claimants were randomly assigned during the third implementation phase of the Enterprise Project. Due to budgetary and other constraints, however, surveys were attempted with only the first 496 sample members to be randomly assigned. Furthermore, since the Cohort 3 implementation period was not concluded until 1993, only one round of surveys (Wave I) was possible with this group.

[89] A complete discussion of these changes was presented in Chapter 4.

The 82 percent survey response rate for Cohort 3 yielded a total sample of 405 for the impact analysis. The analysis of program impacts that follows is based on this sample (213 Treatments and 192 Controls).

As we did above for the Cohort 1 and 2 impact analysis, we first present the self-employment impacts, followed by wage and salary impacts, and finally we present combined self-employment and wage and salary impacts. After completing this sequence of impact analyses, we divide the Cohort 3 sample into two groups: Group 1 is composed of Cohort 3 sample members who filed their UI claim on or before June 1, 1992; Group 2 is composed of Cohort 3 members who filed a UI claim after this date. Claimants who filed their UI claim prior to that date were eligible for a maximum of 30 weeks of UI benefits. Those filing after that date, were limited to 26 weeks of UI benefits. After splitting the Cohort 3 sample into these two subgroups, we re-analyze program impacts for each.

Self-Employment Impacts

In this subsection we present the impacts of the Enterprise Project on self-employment outcomes on the full Cohort 3 sample. As indicated in Table 8.16, the impact of the Enterprise Project on the likelihood of having a self-employment experience during the Wave I observation period was positive and significant. The regression-adjusted logistic impact of 20 percentage points is higher than the 17 percentage point impact reported earlier for Cohorts 1 and 2.

Table 8.16
Self-Employment Impacts
Massachusetts, Cohort 3

	Control	Treatment	Difference	Impact
Percent Self-Employed Since Random Assignment	45%	65%	+20%***	+20%***
Time Self-Employed Since Random Assignment	1.7 months	3.8 months	+2.1*** months	+2.3*** months
Percent Self-Employed at Time of Survey	36%	57%	+21%***	+21%***
Self-Employment Earnings Since Random Assignment	$1,336	$3,243	+$1,907**	+2,138**

The demonstration impact on time employed in self-employment (2.3 months per year) is also higher for Cohort 3 than for Cohorts 1 and 2 (1.0 month per year). Similarly, the impact on the likelihood of employment at the time of the Wave I survey is substantially higher for Cohort 3 than for Cohorts 1 and 2 (21 percentage points versus 10 percentage points).

Perhaps the most dramatic difference, however, is the impact on self-employment earnings. Based on the earlier results for Cohorts 1 and 2, we concluded that the Enterprise Project did not significantly impact self-employment earnings. The results for Cohort 3, however, indicate that the Enterprise Project had a positive and significant impact on self-employment earnings. Specifically, the demonstration increased treatment group members' self-employment earnings by $2,138 per year relative to control group members.

Based on the above impact results, we can conclude that the Cohort 3 self-employment results reaffirmed the earlier self-employment results that were based on Cohorts 1 and 2. In fact, the Cohort 3 results imply even stronger program impacts than were found in the earlier analysis.

Wage and Salary Impacts

In Table 8.17 we present the impacts of the Enterprise Project on wage and salary employment outcomes based on an analysis of the Cohort 3 sample. Here we see that the impact of the Enterprise Project on the likelihood of having a wage and salary job during the Wave I observation period is negative and significant. This result is in contrast to our earlier finding (based on Cohorts 1 and 2) that the Enterprise Project had no impact on the likelihood of wage and salary employment during the Wave I observation period. In fact, the Cohort 3 impact on the likelihood of having a wage and salary job during the Wave I observation period is closer to the Washington SEED results (see Chapter 7).

Table 8.17

Wage and Salary Employment

Massachusetts, Cohort 3

	Control	Treatment	Difference	Impact
Percent Employed in W&S Since Random Assignment	64%	54%	-10%*	-10%**
Time Employed in W&S Since Random Assignment	3.4 months	3.0 months	-0.4 months	-0.5 months
Percent Employed in W&S at Time of Survey	52%	40%	-12%**	-12%***
W&S Earnings Since Random Assignment	$8,385	$5,827	-$2,558**	-$2,732**

The second line in Table 8.17 reports the Enterprise Project impact on time employed per year in wage and salary employment. The results indicate that the Enterprise Project has a negative but insignificant impact on this outcome. Again, this result differs from the earlier (Cohort 1 and 2) results where the impact was positive and significant.

As indicated in the third line of Table 8.17, the Enterprise Project impact on the likelihood of wage and salary employment at the time of the Wave I survey was negative and significant. Again, this result differs from the earlier Enterprise Project impacts and supports the findings from SEED.

Even stronger support for the SEED findings is found in the results on wage and salary earnings. Based on the Cohort 3 results, the Enterprise Project has a negative and significant impact on wage and salary earnings. This result is similar to the SEED results and very different from the earlier Enterprise Project results.

Clearly, the above Cohort 3 results on wage and salary outcomes are perplexing since they differ so dramatically from the earlier results (based on Cohorts 1 and 2). Furthermore, the above results appear to support the SEED findings (reported in Chapter 7).

Total Employment and Earnings Impacts

In Table 8.18 we present the Enterprise Project impacts on combined self-employment and wage and salary employment outcomes based on an analysis of the Cohort 3 sample. A comparison of these impact results with the Enterprise Project results presented earlier in this chapter (based on Cohorts 1 and 2) as well as with the SEED impact results presented in Chapter 7 indicates that the Cohort 3 earnings impacts are more similar to the SEED impact results than they are to the Enterprise Project Cohorts 1 and 2 results. The Cohort 3 impact estimate on total earnings is insignificant, similar to the SEED impact estimate. This is in contrast to the large and significant impact on earnings for Cohort 1 and 2 in Massachusetts.

Table 8.18

Combined Self-Employment and Wage and Salary Employment

Massachusetts, Cohort 3

	Control	Treatment	Difference	Impact
Percent Employed Since Random Assignment	89%	96%	+7%**	+7%**
Time Employed Since Random Assignment	5.3 months	7.1 months	+1.8*** months	+1.9*** months
Percent Employed at the End of Observation Period	79%	88%	+9%**	+9%**
Earnings Since Random Assignment	$10,493	$10,358	-$135	+$134

Other Impacts

In Table 8.19, we present the Cohort 3 program impacts on length of the first UI spell and on the amount of UI benefits received during the first benefit year. The results for Cohort 3 are substantially different from the results reported earlier for Cohorts 1 and 2. Whereas for Cohort 1 and 2 we reported that the demonstration significantly reduced the length of the first UI spell (see Table 8.14), we now find no statistically significant impact.

Similarly, where previously we found that the Massachusetts Demonstration significantly reduced UI benefits in the first benefit year, we now find no statistically significant impact. These UI impact results, together with the other Cohort 3 impact results on employment and earnings, suggest a dramatic difference in program impacts between Cohorts 1 and 2 and Cohort 3.

Table 8.19

UI Impacts

Massachusetts, Cohort 3

	Control	Treatment	Difference	Impact
Length of First UI Spell	23.7 weeks	24.4 weeks	0.7 weeks	0.3 weeks
UI Benefits in First Benefit Year	$6,250	$6,517	$267	$211

Impacts for Various Claimant Subgroups

As part of our analysis of program impacts, we examined whether the impacts of the Enterprise Project varied by claimant characteristics, including gender, race, site, business experience and whether individuals intended to return to their previous employers at the time of application to the demonstration. To test for differential program impacts across subgroups, we added interaction terms (e.g., treatment status interacted with subgroup characteristic(s)) to the basic multivariate regression model described earlier in this chapter. In this section we describe the general patterns of the subgroup results for the main outcome measures derived from the followup survey data that were statistically significant in both Wave I and Wave II; readers interested in additional details can refer to Appendix C.

First, we examined the patterns of subgroup impact differences for the self-employment outcome measures. We examined whether any interaction effect was significant in both Waves

I and II. The results of this analysis yielded no persistent interaction effect for both observation periods. Thus, we conclude that the Massachusetts demonstration impacts on self-employment outcomes were widespread and not concentrated on any of the subgroups tested.

The Enterprise Project did, however, have a differential subgroup effect on one wage and salary outcome measures. Specifically, for males, the demonstration reduced the likelihood of having a wage and salary job during both observation periods. There were no other significant interaction effects that persisted in both observation periods.

Finally, we examined whether there were significant interaction effects on the combined self-employment and wage and salary employment outcome measures. Again, for these outcomes we found that all the impacts are widespread and not concentrated in specific subgroups. None of the interaction terms were significant in both Waves I and II.

SUMMARY

In this chapter we presented our detailed results concerning the impacts of the Enterprise Project on numerous self-employment and wage and salary employment outcome measures, as well as on job creation and UI benefits receipt. In this section, we briefly summarize the highlights of these findings. In particular, we focus on the main self-employment and wage and salary employment and earnings measures based on the followup surveys and on the receipt of UI benefits that contribute to the benefit-cost results described in Chapter 10.

The main impact results of the Enterprise Project are summarized in Table 8.20. As these results indicate, the Enterprise Project had positive impacts on only some of the self-employment outcomes analyzed. Moreover, the impacts appear to be somewhat greater in Wave I than in Wave II. Specifically, in both observation periods, treatment group members were more likely than controls to have a self-employment experience (by about 12-17 percentage points) and to spend more time per year in self-employment (by 0.8-1.0 months). At the time of the Wave I survey, treatment group members were also more likely than controls to be self-employed (by 11 percentage points). The demonstration, however, did not have a significant impact on self-employment earnings during either of the two observation periods.

Consistent with the results on self-employment outcomes, the impacts on wage and salary outcomes were greater in Wave I than in Wave II. For example, in Wave I, the Enterprise Project increased claimants' time in wage and salary employment by nearly one month per year; it also increased the likelihood of working in wage and salary employment at the time of the survey by 6 percentage points. In Wave II, however, the impact of the demonstration on these outcomes was insignificant. In contrast, the impact of the Enterprise Project on earnings from wage and salary employment persisted in both observation periods. Specifically, the demonstration significantly increased wage and salary earnings by over $3,000 per year in both Waves I and II.

In the bottom panel of Table 8.20 we present the impacts of the Enterprise Project on total employment and earnings outcomes. These combined results are large and consistent across both observation periods. Specifically, we find that the program significantly increased the likelihood of claimants' finding employment in both observation periods (by 5-11 percentage points), increased the number of months worked per year (by 1.9-2.1 months per year), and increased the likelihood of being employed by 13 percentage points at the time of the Wave I survey and 6 percentage points at the time of the Wave II survey. Perhaps the greatest impact, however, is on total earnings. The demonstration increased combined annual earnings by nearly $5,000 in Wave I and by nearly $6,000 in Wave II. Thus, it appears that the Enterprise Project had a dramatic positive impact on claimants' total earnings.

We also examined in detail the impacts of the demonstration on UI benefits receipt. Although not reported in this table, our results indicate that the demonstration decreased receipt of UI benefits by $768 during the initial benefit year. Over the entire 31-month observation period, our results indicate that the Enterprise Project decreased UI benefits received by claimants by $876.

Table 8.20
Massachusetts Summary
Program Impacts
Cohorts 1 & 2

	Wave I	Wave II
Self-Employment		
Percent Self-Employed Since Random Assignment	+17%***	+12%**
Time Self-Employed Since Random Assignment	+1.0*** months	+0.8* months
Percent Self-Employed at the End of Observation Period	+11%**	+5%
Self-Employment Earnings Since Random Assignment	+$1,218	+$1,219
Wage and Salary Employment		
Percent Employed in W&S Since Random Assignment	+1%	-4%
Time Employed in W&S Since Random Assignment	+0.8** months	+0.6 months
Percent Employed in W&S at the End of Observation Period	+6%*	+1%
W&S Earnings Since Random Assignment	+$3,230**	+$3,053**
Total Employment		
Percent Employed Since Random Assignment	+11%***	+5%**
Time Employed Since Random Assignment	+2.1*** months	+1.9*** months
Percent Employed at the End of Observation Period	+13%***	+6%*
Total Earnings Since Random Assignment	+$4,764**	+$5,940***

9

COMPARISON OF DEMONSTRATION IMPACTS

The impacts of the Washington demonstration were presented in detail in Chapter 7; the impacts of the Massachusetts demonstration were presented in Chapter 8. This chapter serves two related purposes: to consolidate and summarize the impact results that were presented in the previous two chapters, and to identify impact differences between the two demonstrations.

To meaningfully compare program impacts, it is important to take account of the different population groups served by the two demonstrations. Only when the population differences are accounted for can we be sure that the impact differences observed actually reflect program differences. This chapter describes the procedures that were used to account for the population differences and to generate a new Washington analysis sample.

Using this new Washington analysis sample, the SEED demonstration impacts are reestimated. These reestimated Washington impacts are then compared to the Massachusetts impacts. The details of the process used to generate a new analysis sample in Washington are described in the following section. Then, using the same format as was used in the previous two chapters, we first discuss impacts of the demonstrations on self-employment; next, we discuss impacts on wage and salary employment. We then combine self-employment and wage and salary employment outcomes and compare the demonstrations' impacts on total employment and total earnings. We conclude the chapter with a comparison of impacts on job creation, duration of UI spells, and UI benefits receipt.

Identification of a Washington Comparison Sample

The Washington and Massachusetts self-employment demonstrations served somewhat different claimant target groups. The Washington SEED Demonstration was designed to serve a broad group of new UI claimants who were not job attached. It did not restrict program eligibility to potential applicants based on the number of weeks of UI available to claimants, nor

did it target individuals who were likely to be long-term unemployed. In contrast, the Massachusetts Enterprise Project limited entry to the demonstration to those who were eligible for at least 26 weeks of UI payments, and used a profiling model approach to restrict the program to claimants most likely to exhaust their available unemployment insurance benefits. To simulate what the impacts of the SEED Demonstration would have been had the Massachusetts targeting philosophy been applied, we imposed similar program eligibility requirements to the SEED sample and re-calculated program impacts for the restricted sample for comparison with the Massachusetts impact results. Below we describe our approach to transform the SEED claimant sample to make it more similar to the Massachusetts sample.

The process of developing a restricted SEED sample that generally mirrored the target group of UI claimants served by the Massachusetts Enterprise Project involved four steps. First, we restricted the SEED sample to those who were eligible for at least 26 weeks of UI benefit payments. Among the original sample of 752 controls and 755 treatment group members, 70.5 percent of controls and 70.2 percent of treatments were eligible for at least 26 weeks of UI benefits. By imposing this restriction, the original SEED sample of 1,507 claimants was reduced to 1,060, with 530 claimants each in the treatment and control groups.

The second step, similar to the approach used in Massachusetts, involved estimating a model to identify claimants likely to be long-term unemployed. To develop such a model, we used data from a 10 percent random sample of all new claimants in the six SEED sites during the demonstration period. Of the over 4,200 claimants in the random sample, we first excluded full-referral union members and claimants on standby (i.e., temporary layoff) to incorporate key targeting requirements imposed by SEED. We then also excluded claimants who were not eligible for at least 26 weeks of UI benefits, as well as those who did not receive at least one UI payment. Like the target group for the Massachusetts demonstration, this yielded a sample of new claimants who were not job attached, who had received some UI payments, and who were eligible for enough UI benefits that they had to be unemployed for a long period to exhaust

available UI payments. Among this sample, we then estimated a probit regression model to predict the likelihood of exhausting the UI claim.[90]

The third step involved applying the model to calculate the predicted probability of exhausting the UI claim for each claimant in the restricted sample. The predicted value of the likelihood of exhaustion from the profiling model ranged among claimants from a low of 6.8 percent to a high of 58.0 percent; the mean value was 27.2 percent. The distribution of predicted values was also very similar for treatment and control group members.

The fourth and final step involved eliminating from the sample the treatment and control group members who were least likely to exhaust their available UI benefits. The approach used in Massachusetts established a threshold value for the predicted probability of exhaustion and excluded all claimants who fell below the threshold. As described earlier, this approach resulted in excluding only 12 percent from the target pool of interested claimants who had received some UI and were eligible for at least 26 weeks of UI benefits. To mirror as closely as possible the approach used in Massachusetts to select the program's target group, we excluded from the SEED sample of 1,060 the 12 percent with the lowest predicted probability of exhaustion, resulting in a final trimmed analysis sample of 933.

Below we present the impact results based on this trimmed sample. We also compare the SEED impact results from the trimmed sample with both the SEED results from the original sample and with the Massachusetts demonstration results.

IMPACTS ON SELF-EMPLOYMENT

In this section, we compare the effects of the two demonstrations on the self-employment experiences of program participants. We examine the impact of the two demonstrations on the

[90]The explanatory variables included in the probit model included a set of four dummy variables to capture claimant's education level, a set of eight occupation dummy variables based on 1-digit codes of the Dictionary of Occupational Titles, wages earned in covered employment in the four complete quarters before filing the UI claim, the wage replacement rate (i.e., the proportion of prior earnings that the weekly benefit amount represents on an annual basis), and a set of five site dummy variables.

likelihood of entering self-employment, the time spent in self-employment, the likelihood of being self-employed at the time of the followup surveys, and the earnings from self-employment.

Impacts on Likelihood of Self-Employment Experience

In Table 9.1, we present the impacts of the two demonstrations on the likelihood of having at least one self-employment experience during the Wave I and Wave II observation periods.[91] The results indicate that, in both states, treatment group members were significantly more likely than control group members to have at least one self-employment experience since random assignment. During Wave I, the Massachusetts demonstration increased the likelihood of having a self-employment experience by 17 percentage points (or by 49 percent of the control group mean). The Washington demonstration's impact was even more dramatic, increasing the likelihood by 28 percentage points (or by 80 percent of the control group mean).

Table 9.1

Self-Employment Experience Since Random Assignment

	Control	Treatment	Difference	Impact
Wave I				
Massachusetts	35%	52%	+17%***	+17%***
Washington	35%	63%	+28%***	+28%***
Wave II				
Massachusetts	47%	58%	+11%**	+12%**
Washington	44%	66%	+22%***	+22%***

[91]The reader should note that the observation periods are not identical in the two demonstrations (refer to Chapter 6 for details). In Massachusetts, the Wave I and Wave II observation periods are approximately 19 months and 31 months, respectively. In Washington, the Wave I and Wave II observation periods are approximately 21 months and 33 months, respectively. Some of the impact differences may thus be attributable to the observation period differences. The time and earnings variables, however, have been annualized, thus, eliminating the influence of different observation periods.

In comparison to the Wave I impacts, the Wave II impacts were somewhat smaller. In Massachusetts, the impact fell to 12 percentage points (from 17 percentage points); in Washington, the impact fell to 22 percentage points (from 28 percentage points).[92]

The smaller impacts in Wave II (as compared with Wave I) suggests that the demonstrations had two types of effects. The first effect was to expedite entry into self-employment among UI claimants who would have entered self-employment even without the demonstration. This first type of effect does not permanently alter the self-employment entry rate among UI claimants; it only alters *when* these individuals enter self-employment.[93] The second type of effect was to encourage entry into self-employment among UI claimants who might never have considered self-employment without the demonstration.

One indicator that these two types of effects exist is found in the results presented in Table 9.1. That is, when we analyze the impact of the demonstration on the likelihood of having a self-employment experience over a relatively short time frame (e.g., Wave I), the impact of the demonstration is relatively large since it includes both transitory and permanent effects. As the time frame is increased (e.g., from Wave I to Wave II), the transitory effect dissipates (since nearly everyone who is likely to enter self-employment has done so) and only the permanent effect remains. This phenomenon helps to explain the reduction in impacts (from Wave I to Wave II) identified in Table 9.1.

We speculate that with nearly a three-year observation period (Wave II), the transitory effect of expedited entry has largely dissipated. What remains is the permanent effect of the demonstration on entry into self-employment among UI claimants. Thus, the Wave II impacts

---

[92]The combined effect of excluding claimants who were eligible for fewer than 26 weeks of UI benefits and applying the exhaustion algorithm in Washington, can be derived by comparing the results of Table 7.1 with the results of Table 9.1. Using the full Washington sample (Table 7.1), the Washington demonstration increased the likelihood of having a self-employment experience since random assignment by 25 percentage points during Wave I and 22 percentage points during Wave II. The results in Table 9.1, indicate that the impacts were slightly higher when using the more restrictive sample (i.e., those eligible for 26 or more weeks of UI benefits and those most likely to exhaust their benefits). Specifically, during Wave I, the impact was 28 percentage points; while, during Wave II, the impact was 22 percentage points.

[93]Previous analysis (Benus, Wood, and Grover, 1994) showed that SEED expedited entry into self-employment by approximately six months, while the Enterprise Project expedited entry into self-employment by about two months.

may be thought of as the long-term impacts of the demonstrations on the likelihood of self-employment.

Impacts on Time in Self-Employment

Another dimension of the self-employment experience is the time spent in self-employment. In Table 9.2 we compare the demonstrations' impacts on annual time spent in self-employment. The impacts are significant in both demonstrations as well as in both observation periods. In Washington, the impacts are substantially larger than in Massachusetts. In Wave II, for example, the Washington demonstration increased the time in self-employment by over two months per year, whereas the Massachusetts demonstration increases the time in self-employment by less than one month per year. These results indicate that, over a three-year period, the Washington demonstration increased total time in self-employment by about six[94] months; in contrast, the Massachusetts demonstration increased total time in self-employment by slightly over two months.[95]

Impacts on Self-Employment at Time of Followup Surveys

The above results indicate that both demonstrations significantly increased the likelihood of participants' entry into self-employment and the time spent in self-employment. Another indicator of program success is the likelihood of being self-employed at some fixed point in time following random assignment. In Table 9.3 we present the demonstration impacts on the likelihood of being self-employed at the time of the followup surveys.

As indicated in Table 9.3, both demonstrations had large and positive impacts on the likelihood of self-employment at the time of the Wave I survey. In Massachusetts, the impact was 11 percentage points (38 percent of the control group mean); in Washington, the impact was

[94]This estimate is derived by multiplying the Wave II impact estimate (2.3 months) by 33/12. The Massachusetts estimate is derived by multiplying the impact estimate (0.8) by 31/12.

[95]A comparison of the full-sample results (Table 7.2) with the results in this chapter (based on the likely exhaustees eligible for 26 or more weeks of UI benefits) indicates that the Washington demonstration had sightly smaller impacts on the full sample than it did on the more restrictive sample (Table 9.2).

Table 9.2

Annual Time in Self-Employment Since Random Assignment

	Control	Treatment	Difference	Impact
Wave I				
Massachusetts	1.4 months	2.5 months	+1.1*** months	+1.0*** months
Washington	1.1 months	3.8 months	+2.7*** months	+2.8*** months
Wave II				
Massachusetts	1.7 months	2.6 months	+0.9** months	+0.8* months
Washington	1.1 months	3.4 months	+2.3*** months	+2.3*** months

Table 9.3

Self-Employment at Two Different Observation Times

	Control	Treatment	Difference	Impact
Wave I				
Massachusetts	29%	40%	+11%**	+11%**
Washington	24%	43%	+19%***	+19%***
Wave II				
Massachusetts	38%	43%	+5%	+5%
Washington	27%	40%	+13%***	+13%***

19 percentage points (79 percent of the control group mean). At the time of the Wave II survey, however, the Massachusetts demonstration impact declined to only 5 percentage points which was not significant. The Washington demonstration impact also declined somewhat, but remained significant at the .01 level (13 percentage points).[96]

Impacts on Self-Employment Earnings

The results of Table 9.4 indicate that, in Massachusetts, the demonstration did not have a significant impact on annual self-employment earnings in either the Wave I or Wave II observation periods. In contrast, the Washington impacts were positive and significant in both observation periods.

Table 9.4

Annual Earnings from Self-Employment Since Random Assignment

	Control	Treatment	Difference	Impact
Wave I				
Massachusetts	$1,404	$2,916	+$1,512	+$1,218
Washington	$815	$2,979	+$2,164**	+$2,003*
Wave II				
Massachusetts	$1,439	$2,627	+$1,188	+$1,219
Washington	$703	$3,029	+$2,326**	+$2,157**

It is worth noting that the magnitude of the impacts are approximately constant across both observation periods. In Massachusetts, for example, the estimated impacts (while not significant at conventional levels) were approximately $1,200 per year in both observation

[96]Again, the full-sample results (Table 7.3) are slightly smaller than the results presented in Table 9.3 (based on the more restrictive sample of likely exhaustees eligible for 26 or more weeks of UI benefits).

periods. In Washington, the impacts were approximately $2,000 per year during both observation periods.[97]

To summarize the above self-employment results, we found that both demonstrations significantly increased: the likelihood of having a self-employment experience; the time in self-employment; and the likelihood of being self-employed at the time of the Wave II followup survey. A comparison of the magnitude of these impacts between the two demonstrations indicates that SEED consistently had greater impacts on self-employment outcomes than the Enterprise Project. SEED also significantly increased the earnings from self-employment while the Enterprise Project did not.

Impacts on Wage and Salary Employment

Both demonstrations directed their efforts toward enhancing participants' self-employment outcomes. Nonetheless, wage and salary outcomes were also affected by demonstration services. In this section, we compare the impacts of the demonstrations on the likelihood of having a wage and salary job, the time spent in wage and salary jobs, the likelihood of having a wage and salary job at the time of the followup surveys, and earnings from wage and salary jobs.

Impacts on Likelihood of Wage and Salary Employment Experience

In Table 9.5, we present a comparison of the demonstration impacts on the likelihood of having a wage and salary job during the two observation periods. In Massachusetts, the likelihood of having a wage and salary job during both observation periods was not affected by the demonstration. That is, we found no statistical difference between the treatment and the control groups in the likelihood of having a wage and salary job. In Washington, on the other hand, treatment group members were significantly less likely than control group members to have a wage and salary job during both observation periods. These results suggest that while the Massachusetts demonstration did not affect the wage and salary employment prospects of

[97]The reader should recall that the Washington impacts are substantially influenced by one outlier. This single case increased the Washington estimated annual impacts on self-employment earnings by approximately $800.

treatment group members, the Washington demonstration significantly reduced the likelihood of wage and salary employment of treatment group members.[98]

Table 9.5

Wage and Salary Employment Experience Since Random Assignment

	Control	Treatment	Difference	Impact
Wave I				
Massachusetts	64%	65%	+1%	1%
Washington	80%	71%	-9%***	-9%***
Wave II				
Massachusetts	75%	71%	-4%	-4%
Washington	86%	78%	-8%***	-7%***

Impacts on Time in Wage and Salary Jobs

In Table 9.6, we present impact estimates on the time spent per year in wage and salary employment since random assignment. In Washington, treatment group members spent nearly one month less per year in wage and salary employment than controls (in both observation periods); in Massachusetts, on the other hand, treatment group members spent nearly one month more per year than controls in wage and salary employment (only the Wave I impacts are significant).[99]

For Washington, these results are consistent with the findings on the likelihood of having a wage and salary job experience (see Table 9.5). That is, the Washington demonstration significantly decreased both the likelihood of having a wage and salary job and the time spent

[98]A comparison of these results with the Washington full sample results (presented in Table 7.5) indicates that the full-sample results are slightly smaller.

[99]The Washington full-sample results in Table 7.6 are slightly smaller than the results based on the more restrictive Washington sample (presented in Table 9.6).

Table 9.6

Time in Wage and Salary Employment Since Random Assignment

	Control	Treatment	Difference	Impact
Wave I				
Massachusetts	3.2 months	3.8 months	+0.6 months	+0.8** months
Washington	5.0 months	4.2 months	-0.9** months	-0.9** month
Wave II				
Massachusetts	4.1 months	4.4 months	+0.3 months	+0.6 months
Washington	5.4 months	4.6 months	-0.8** months	-0.8** months

in such employment during both observation periods. For Massachusetts, on the other hand, the results are mixed. That is, the Massachusetts demonstration did not affect the likelihood of having a wage and salary job in either Wave I or Wave II; it did however, have a positive impact on the time in such employment during Wave I. Thus, in contrast to the negative impacts on wage and salary employment outcomes in Washington, the Massachusetts demonstration appears to have increased time in wage and salary employment in the short-term.

Impacts on Wage and Salary Employment at Time of the Followup Surveys

Table 9.7 presents the impact estimates on the likelihood of wage and salary employment at the time of the two followup surveys. In Washington, the demonstration significantly decreased the likelihood of wage and salary employment at the time of the Wave I followup

survey; at the Wave II survey, however, the impact was insignificant. In Massachusetts, there was no significant impact at either survey.[100]

Table 9.7

Wage and Salary Employment at Two Different Observation Times

	Control	Treatment	Difference	Impact
Wave I				
Massachusetts	46%	52%	+6%	+6%
Washington	65%	56%	-9%***	-9%***
Wave II				
Massachusetts	54%	56%	+2%	+1%
Washington	64%	60%	-4%	-4%

Impacts On Wage and Salary Earnings

The impacts on annual wage and salary earnings during the two observation periods are presented in Table 9.8. In Massachusetts, the demonstration significantly increased wage and salary earnings during both observation periods. Indeed, treatment group members earned approximately $3,000 more per year from their wage and salary jobs than their control group counterparts. In Washington, on the other hand, treatment group members earned approximately $2,000 less in Wave I and approximately $1,700 less in Wave II than their control group counterparts. This is perhaps the most dramatic difference in the impacts of the two demonstrations.

One explanation for this dramatic difference in impacts may be found in the design and implementation of the demonstrations. As described in earlier chapters, the Washington

[100]The Washington full-sample results, presented in Table 7.7, are slightly smaller than the results presented in Table 9.7.

Table 9.8

Earnings from Wage and Salary Employment Since Random Assignment

	Control	Treatment	Difference	Impact
Wave I				
Massachusetts	$6,613	$9,311	+$2,698**	+$3,230**
Washington	$10,586	$8,661	-$1,925	-$2,064**
Wave II				
Massachusetts	$7,797	$10,119	+$2,322*	+$3,053**
Washington	$11,041	$9,323	-$1,718	-$1,744*

demonstration provided the business assistance training and much of the counseling during a concentrated, brief period soon after selection into the demonstration. In contrast, the Massachusetts demonstration provided business assistance training and counseling over a much longer time period. The training modules in Washington were presented during a one-week period; in contrast, the Massachusetts training seminars were spread over a twelve-week period. Similarly, treatment group members in Washington received on average 1.5 hours of individual counseling; in contrast, Massachusetts' treatment group members received on average, 7.5 hours of counseling.

These differences in program design and implementation may partly explain the difference between the two demonstrations in wage and salary earnings impacts. That is, the longer contact period in Massachusetts between participants and their trainers/counselors may have contributed to the more favorable wage and salary earnings outcomes in Massachusetts. That is, in Massachusetts, counselors were able to spend more time (than in Washington) with their clients to assess whether self-employment remained a viable option after observing the client's progress in developing the business idea. In some situations, the counselor may have helped the client determine that wage and salary employment was the best option available. While counselors in Massachusetts may not have worked directly on helping treatment group members find wage and salary jobs, the substantial time that they spent with their clients may

have resulted in improved wage and salary outcomes for their clients.[101] Clearly, this explanation of the difference in wage and salary outcomes between the two demonstrations is speculative. It represents only one of many possible explanations.

Impacts on Total Employment and Earnings

In the previous sections, we discussed the impacts of the Massachusetts and Washington demonstrations on a number of self-employment outcomes as well as on a number of wage and salary outcomes. In this section we combine the self-employment and wage and salary outcomes and present impact estimates for the combined outcomes. We present the following four impact estimates for both demonstrations and both observation periods: the likelihood of having either a wage and salary or a self-employment experience; the time employed per year; the likelihood of being employed at the time of the followup surveys; and the total earnings per year from either type of employment.

Impacts on Likelihood of Employment Experience

Table 9.9 presents the estimated impacts of the Washington and Massachusetts demonstrations on the likelihood of having had either a wage and salary job or a self-employment spell during the two observation periods. The results indicate that, in Massachusetts, the demonstration had a positive and significant impact during both observation periods. In contrast, the Washington demonstration had a significant impact during Wave I only.[102] However, given the high level of control group employment in Wave II (especially in Washington where 97 percent of the control group had an employment experience during the observation period), it is extremely difficult to detect a significant increase.

---

[101]The Washington Wave II impacts are similar for both the full-sample (presented in Table 7.8) and for the more limited sample of likely exhaustees who were eligible for 26 or more weeks of UI benefits (presented in Table 9.8). The Wave I impacts, however, are different in the two samples. The impact for the full-sample is -$1,407 while the impact for the restricted sample is -$2,064.

[102]An examination of Table 7.9 indicates that, for the full Washington sample, the results were also significant for Wave I only.

Table 9.9

Combined Employment Experience Since Random Assignment

	Control	Treatment	Difference	Impact
Wave I				
Massachusetts	82%	92%	+10%***	+11%***
Washington	93%	96%	+3%*	+3%*
Wave II				
Massachusetts	92%	97%	+5%**	+5%**
Washington	97%	98%	+2%*	+2%

Impacts on Time in Employment

Impacts on total time employed are presented in Table 9.10. In both demonstrations, the impacts on time employed are positive and significant. Specifically, the Massachusetts demonstration increased total time in employment by approximately two months per year during both Waves I and II. The Washington demonstration had a similar impact during Wave I, but

Table 9.10

Total Time in Employment Since Random Assignment

	Control	Treatment	Difference	Impact
Wave I				
Massachusetts	4.5 months	6.5 months	+2.0*** months	+2.1*** months
Washington	6.2 months	7.4 months	+1.2*** months	+1.3*** months
Wave II				
Massachusetts	5.8 months	7.4 months	+1.6*** months	+1.9*** months
Washington	6.7 months	7.8 months	+1.2*** months	+1.1*** months

a smaller impact (approximately one month increase per year) during Wave II. Thus, both demonstrations significantly increased the total time employed during both observation periods.[103]

Impacts on Employment at Time of the Followup Surveys

The impacts on the likelihood of having a wage and salary job and/or being self-employed at the time of the follow-up surveys are presented in Table 9.11. These impacts are somewhat surprising. At the time of the Wave I survey, the Massachusetts impact was positive and significant, while the Washington impact was insignificant. At the time of the Wave II survey, however, the impacts were quite similar: both the Massachusetts and Washington impacts were positive and significant.

Table 9.11

Employment at Two Different Observation Times

	Control	Treatment	Difference	Impact
Wave I				
Massachusetts	68%	81%	+13%***	+13%***
Washington	81%	84%	+3%	+3%
Wave II				
Massachusetts	81%	87%	+6%	+6%*
Washington	83%	88%	+5%*	+5%**

Thus, the results of Table 9.11 indicate that the Massachusetts impacts dissipated over time, while the Washington impacts increased. It is relatively easy to explain the Massachusetts pattern of program impacts dissipating over time. It is much more difficult, however, to explain the Washington pattern of program impacts.

One explanation for the Washington result is the significant and opposite effects of SEED on the likelihood of self-employment and wage and salary employment. That is, SEED had a

[103]The Washington results for the full-sample indicate smaller impacts in time in employment.

positive impact on the likelihood of self-employment (Table 9.3) and a negative impact on the likelihood of wage and salary employment (Table 9.5). These opposite impacts combined to yield an insignificant impact on the likelihood of employment at the time of the Wave I survey. At the time of the Wave II survey, however, the strong positive impact on the likelihood of self-employment dominated the smaller negative impact on the likelihood of wage and salary employment.

Impacts On Earnings

The earnings impacts of the two demonstrations are presented in Table 9.12. As indicated by the results, the impacts are dramatically different for the two demonstrations. In Massachusetts, the impacts are positive and significant for both observation periods. Indeed, the results indicate that the Enterprise Project increased treatment group members' earnings by more than $4,700 per year during Wave I, an increase of over 50% of the control group mean earning levels. During Wave II, the impact was even greater, increasing total earnings by nearly $6,000 per year. In contrast, SEED did not have a significant impact on total earnings in either observation period.

Table 9.12

Combined Earnings from Employment Since Random Assignment

	Control	Treatment	Difference	Impact
Wave I				
Massachusetts	$8,483	$13,151	+$4,668**	+$4,764**
Washington	$12,256	$12,943	+$687	+$256
Wave II				
Massachusetts	$10,056	$14,664	+$4,608**	+$5,940***
Washington	$13,173	$14,259	+$1,086	+$205

The earnings impact estimates for Massachusetts are dramatic and extremely interesting. In Massachusetts, the two earnings impacts (wage and salary earnings impact and self-employment earnings impact) reinforce each other. That is, the Enterprise Project has positive

(but insignificant) impacts on self-employment earnings and positive (and significant) impacts on wage and salary earnings. The positive total earning impacts in Massachusetts are thus supported by both wage and salary and self-employment earnings but driven mainly by the large impacts of the Enterprise Project on wage and salary earnings.

In contrast, the two earnings impacts in Washington do not reinforce each other, but rather, they offset each other. That is, while SEED has a positive and significant impact on self-employment earnings, it has an offsetting negative and significant impact on wage and salary earnings. The impact on combined earnings is thus canceled and yields insignificant total earnings impacts.

Impacts on Job Creation

Above, we estimated the impacts of the Enterprise Project and SEED on the employment and self-employment experiences of program participants. We found, for example, that, in both demonstrations, treatment group members were more likely than control group members to be self-employed; we also found that they were employed for a larger fraction of the observation period than their control group counterparts. These findings represent the demonstrations' direct impacts on the employment experiences of participants.

The demonstrations may also have indirect impacts on employment. That is, by increasing the number of businesses, the demonstration may create new jobs for nonparticipants. In the analysis below we consider these new jobs as additional impacts of the demonstrations. It is important to note that this impact analysis may overstate the true impact on employment since attributing all these jobs to the demonstrations, implicitly assumes that these "new jobs" did not displace other jobs.

For our analysis of the impacts on job creation, we combine the number of family members and nonfamily members employed in each business to obtain a total number of employees in each business. We measure the effect of the demonstrations on nonparticipant employment as the difference between the total number of employees in treatment group businesses and the total number of employees in control group businesses.[104]

[104] Total number of employees includes all family and nonfamily wage and salary employees excluding the business owner(s).

As indicated in Table 9.13, in Massachusetts, there was no statistically significant impact on nonparticipant employment. In Washington, there was also no significant impact on job creation during Wave I. During Wave II, however, SEED had a positive and significant impact on job creation. One explanation for this finding, is that, in Washington, treatment group businesses had grown to the point where they were employing nonparticipants.

Table 9.13

Job Creation Impacts: Total Employment

	Control	Treatment	Difference	Impact
Wave I				
Massachusetts	0.7	0.4	-0.4	-0.4
Washington	0.2	0.3	+0.1	+0.1
Wave II				
Massachusetts	1.7	0.8	-0.9	-1.0
Washington	0.2	0.4	+0.3***	+0.3***

IMPACTS ON UI OUTCOMES

In previous chapters we found that the demonstrations also have significant impacts on UI outcomes of program participants. In this section we compare the demonstrations' impacts on selected UI outcomes, including length of first UI spell and amount of UI benefits received.

Using data from the UI history files, we developed three main measures of UI benefit receipt: number of weeks of the first spell of UI receipt; total amount of UI benefits received during the first benefit year; and total amount of UI benefits received during the Wave II observation period.

As shown in Table 9.14, the impact of the Massachusetts demonstration was to reduce the length of the first UI spell by approximately two weeks. The impact of the Washington demonstration was a reduction of approximately eight weeks. Thus, while both demonstrations significantly reduced the length of the first UI spell, the impact in Washington was greater. The

greater impact in Washington is largely due to the large number of Washington treatment group members who entered self-employment early in their unemployment spell following receipt of the lump-sum payment.

Table 9.14

Impacts on UI Outcomes

	Control	Treatment	Difference	Impact
Length of First UI Spell				
Massachusetts	26.5 weeks	24.5 weeks	-2.0*** weeks	-1.8*** weeks
Washington	19.3 weeks	11.6 weeks	-7.7*** weeks	-7.6*** weeks
Benefit Amount in First Year				
Massachusetts	$6,845	$6,120	-$725***	-$768***
Washington (UI only)	$4,361	$2,555	-$1,806***	-$1,796***
Washington (UI + Lump-Sum)	$4,361	$5,787	+$1,426***	+$1,451***
Benefit Amount in Wave II Observation Period				
Massachusetts	$7,400	$6,567	-$833***	-$876***
Washington (UI only)	$5,442	$3,517	-$1,925***	-$1,946***
Washington (UI + Lump-Sum)	$5,442	$6,750	+$1,308***	+$1,300***

The second panel of Table 9.14 presents the program impacts on UI benefits received during the first benefit year. The Massachusetts demonstration had significant negative impacts on UI benefits during the first benefit year. Treatment group members in Massachusetts

received approximately $750 less in UI benefits than control group members. Thus, the implementation of Enterprise Project saved money for the UI Trust Fund.

In Washington, the demonstration also had a negative and significant impact on benefits receipt (excluding the lump-sum payment). That is, SEED significantly reduced UI benefits received by treatment group members relative to control group members (by over $1,800). However, taking into account both UI benefit payments plus lump-sum payments yields a very different impact estimate.[105] As seen in the Table 9.14, treatment group members on average received approximately $1,450 more than control group members in total benefits (i.e., UI benefits plus lump-sum payment) during the first year. Thus, whereas the Massachusetts impact estimate indicates a reduction in total payments to program participants, the Washington impact estimate indicates an increase in total payments to program participants in the first benefit year.

An analysis of benefits received during the Wave II observation period reveals similar results. In Massachusetts, treatment group members continued to collect less than their control group counterparts; indeed, for the entire Wave II period, the impact grew to -$876. In Washington, there was a similar pattern, indicating that treatment group members collected less UI benefits than their control group counterparts in the years following their program participation. Specifically, without the lump-sum payment, the demonstration impact was a net reduction in UI benefit payments of -$1,946; including the lump-sum payment, the impact became a net increase in total payments of $1,300.

[105] Approximately 60% of the Washington treatment group received a lump-sum payment. The average lump-sum payment received was $4,225.

10

BENEFITS AND COSTS OF THE SELF-EMPLOYMENT DEMONSTRATIONS

In previous chapters we presented impact estimates for the Massachusetts and Washington UI Self-Employment Demonstrations. The impact results presented in Chapters 7, 8, and 9 indicate that both demonstrations had beneficial effects on a number of outcomes. Impact estimates, however, even when they indicate beneficial program effects, are not sufficient to conclude that a program is worthwhile for society. To conclude that a program is worthwhile for society, the beneficial effects of a program must outweigh its costs.

In this chapter we address the question of whether or not UI self-employment programs are worthwhile for society by comparing the net benefits of the Massachusetts and Washington self-employment demonstrations with their net costs. The Washington net benefits were presented in Chapter 7 and the Massachusetts net benefits were presented in Chapter 8. The net costs are derived from administrative records and from cost data collected from program administrators.

We begin this chapter by discussing the conceptual framework for the benefit-cost analysis. We describe how net social value is calculated from each of four perspectives: that of the participants, nonparticipants, the government sector, and society as a whole. Following this conceptual discussion, we present the estimated benefits and costs for each of the demonstrations. The chapter then concludes with a discussion of the implications of the findings.

200

BENEFIT-COST ANALYSIS

We measure social benefits and costs as changes from what would have occurred in the absence of the demonstration. *Social costs* are defined as the additional resources necessary to provide demonstration services, plus any adverse effects on participants or others that would not have occurred in the absence of the demonstration. Similarly, *social benefits* are defined as the value of all beneficial effects that would not have occurred in the absence of the demonstration. *Net social benefits* are the sum of social benefits and costs. If the incremental social benefits of a demonstration exceed its incremental social costs, net social benefits are positive; if costs exceed benefits, net social benefits are negative.

Social programs are generally viewed as cost-effective if they yield positive net benefits to society as a whole. In assessing the desirability of a program from a policy perspective, however, it is also important to take into account the distributional consequences of the program -- i.e., the benefits and costs to particular groups within society. Thus, we measure the benefits and costs of the demonstration from four different perspectives -- those of the participants, nonparticipants, the government sector, and society as a whole.

Exhibit 10.1 shows how the anticipated demonstration effects can be classified within the social benefit-cost accounting framework from the perspectives of three of the four major groups of interest. In this exhibit, a plus (+) denotes a benefit from the perspective of the group in whose column it occurs, a minus (-) denotes a cost to that group, and a zero (0) indicates that this item is neither a benefit nor a cost to that group. Each effect of the demonstration is classified as a benefit, a cost, or a transfer. "Transfers" are effects that represent a benefit to one group in society and an exactly offsetting cost to another group. For example, the increased taxes paid by program participants on their higher earnings are a cost to the participants. These costs, however, benefit nonparticipants, resulting in no monetary gain or loss to society as a whole.

There are three principal anticipated benefits to participants: earnings gains, improved psychological well-being as a result of being self-employed, and enhanced community economic development. These anticipated benefits are indicated by pluses in the participant column in Exhibit 10.1. In previous chapters, we decomposed program impacts on earnings by considering

Exhibit 10.1

Social Benefits and Costs of the Self-Employment Demonstrations

	Perspective:		
	Society =	**Participants +**	**Nonparticipants**
Benefits			
Earnings and asset gains	+	+	0
· Wage and salary earnings			
· Self-employment earnings			
Improved psychological well-being	+	+	0
Community economic development	+	+	+
Costs			
Demonstration operations	-	0	-
· Administration			
· Services			
Transfers			
UI benefits (including lump-sum payment)	0	+	-
Reduced transfer benefits	0	-	+
Increased tax payments	0	-	+

self-employment earnings and wage and salary earnings separately. For the benefit-cost analysis, this decomposition of total earnings is not relevant -- only impacts on total combined earnings are considered.

Of the above three principal anticipated program benefits, only community economic development is expected to benefit nonparticipants. That is, nonparticipants are expected to benefit from increased employment opportunities and increased tax revenues. The other two anticipated program benefits, earnings gains and improved psychological well-being of participants, are not expected to affect nonparticipants.

Since none of three principal anticipated benefits to participants represent a cost to any other group, they are also benefits to society as a whole. This is indicated in Exhibit 10.1 by pluses in the society column.

The principal social costs of the demonstration are the costs of demonstration operations -- program administration and provision of services. These costs are borne by nonparticipants and represent a cost to society as a whole.

The principal transfers expected to result from the demonstration are the UI benefits (including the lump-sum payment), reduced public transfers to participants, and increased tax payments by participants. Each of these represents a benefit or cost to participants and an exactly offsetting cost or benefit to nonparticipants, and therefore neither a benefit nor a cost from the standpoint of society as a whole. While transfers do not affect the net social benefit of a program, it is nonetheless important that they be measured in order to assess the distributional consequences of the program.

To summarize, from the perspective of program participants, the key benefits of a self-employment program are the increased earnings, improved psychological well-being from being self-employed rather than working for someone else, the enhanced community economic development, and the UI benefits (including the lump-sum payment). On the other hand, the key costs (from the participants' perspective) are the reduced transfer payments received and the increased tax payments.

From the perspective of nonparticipants (i.e., taxpayers), the key benefits are the enhanced community development, reduced transfer payments, and increased tax payments. The key costs, on the other hand, are the expenditures for demonstration operations and the self-employment allowance payments.

Up to this point, we have treated all taxpayers indistinguishably as nonparticipants. Exhibit 10.2 shows the distribution of the benefits and costs of the demonstration within the

Exhibit 10.2

Government Benefits and Costs of the Self-Employment Demonstrations

	Perspective:			
	Government =	**DOL+**	**UI Trust Fund +**	**Other Agencies**
Benefits				
Increased taxes	+	+	+	+
Reduced transfers	+	0	+	+
Costs				
Demonstration operations	-	-	0	0
· Administration · Services				
UI benefits (including lump-sum payment)	-	0	-	0

government sector. As shown in the exhibit, the government sector can be split into the U.S. Department of Labor (DOL), the UI Trust Fund, and all other government agencies. The principal benefits of the demonstration to the government are increased taxes and reduced transfers as a result of participants' enhanced economic well-being.[106]

Both State and Federal taxpayers will benefit from increased FUTA (Federal Unemployment Tax Act) taxes, income taxes, sales taxes, social security contributions, and other revenues generated by increased business activity. State and Federal taxpayers will also benefit from reduced outlays for UI benefits, welfare, and other transfer payments.

[106] Note that outcomes that were designated transfers from a social perspective are classified as benefits when viewed from the government perspective, because the offsetting costs to participants are outside the government sector.

The principal costs of the demonstration to the government are for demonstration operations, including both program administration and services to participants. It is assumed that these costs are borne by DOL. The UI benefits (including the lump-sum payment) represent another principal cost. In our benefit-cost analysis we allocate these costs to the UI Trust Fund.[107] That is, we assume that, in an ongoing program, these payments will be drawn from the UI Trust Fund. This assumption has recently been bolstered by the provisions of the North America Free Trade Agreement (NAFTA) Implementation Act (P.L. 103-182) that authorize States to implement self-employment assistance programs.

One requirement of this legislation (Section 507) is that "the program does not result in any cost to the Unemployment Trust Fund (established by section 904(a) of the Social Security Act) in excess of the cost that would be incurred by such State and charged to such Fund if the State had not participated in such program." Thus, the Act stipulates that benefit payments to UI claimants receiving self-employment assistance are to be paid from the Unemployment Trust Fund. The total amount of these payments, however, may not exceed the total amount that would have been paid in regular UI benefits.

As noted above, the net benefit of the demonstration to any group is the sum of the benefits and costs accruing to that group. In order to derive all-inclusive measures of net benefits, it would be necessary not only to measure all of the benefits and costs of the demonstration, but also to value them in commensurate units, so that they can be summed. The most natural units in which to measure and sum benefits and costs are, of course, dollars. Unfortunately, it may not be possible to measure some demonstration benefits and costs at all, and it will not be possible to value all those that can be measured in monetary terms. For example, improved psychological well-being can at best be measured in nonmonetary terms.

Our approach to benefits that cannot be measured in monetary terms will be as follows. We will first estimate net monetary benefits -- the sum of all benefits and costs that can be measured in monetary terms. If net monetary benefits are positive -- i.e., if monetary benefits

---

[107] It should be noted that in the UI Self-Employment Demonstrations, these costs were not borne by the UI Trust Fund. Rather, in Washington these payments were funded through DOL research funds. In Massachusetts, payments were paid out of the UI Trust Fund; however, Massachusetts was required to reimburse the Fund if it incurred "excess costs" (see Chapter 2).

exceed monetary costs -- there is no need to value nonmonetary benefits in dollar terms, since they can only increase net benefits, whatever their value. (Note that no nonmonetary costs are anticipated.) Alternatively, if monetary costs exceed monetary benefits, then the excess of monetary costs over monetary benefits ("net social cost") is a measure of how valuable the nonmonetary benefits would have to be for the program to be worthwhile -- i.e., for overall net benefits to be positive.

MEASUREMENT OF OUTCOMES

As discussed in the previous section, the principal benefits, costs, and transfers expected to result from the demonstration are earnings gains, improved psychological well-being of participants, enhanced economic community development, the costs of demonstration operations, reduced transfers from UI and other government programs, and increased tax payments. In this section, we discuss how we measure each of these demonstration benefits and costs.

As noted at the outset, social benefits and costs are defined as changes from what would have occurred in the absence of the demonstration. Thus, in general, the benefits and costs of the demonstration will be measured by the difference between the outcomes of the treatment group and those of the control group, which represents what would have happened to the treatment group in the absence of the demonstration. Below, we first discuss how the benefits are defined and measured and then we discuss the definition and measurement of costs.

Program Benefits

As discussed above, the main social benefits of the self-employment demonstrations are earnings gains, improved psychological well-being, and community economic development. Of these three benefits, earnings gains are the most straight-forward to measure. The other two benefits -- psychological well-being and community economic development -- are much more difficult to measure since they are not readily quantifiable in monetary terms.

Measurement of psychological well-being represents an extremely difficult methodological challenge. We evaluated differences in overall job satisfaction between the self-employed and those in wage and salary employment on the basis of responses to job satisfaction questions that

were incorporated into the surveys. We asked both the self-employed and those in wage and salary employment whether or not self-employment is "better" than working for someone else. The results, presented in an earlier report (see Benus, Johnson, and Wood, 1993) showed that treatment group members had greater job satisfaction than controls.

We have not attempted to quantify this nonpecuniary impact of the demonstration more precisely (e.g., in monetary terms). As stated earlier in this chapter, if the net monetary benefits of the demonstrations are positive, then the addition of this nonpecuniary effect will only enhance net benefits and not affect the assessment of whether the program is worthwhile from society's perspective.

The measurement of total community economic development effects of the demonstration is beyond the scope of this evaluation. As a result, we did not attempt to measure the effect of the demonstrations on local economic conditions or on other community-wide outcomes. We did, however, measure several key indicators of the degree to which the demonstration succeeded in enhancing economic development. For example, we measured the net number of new businesses created as a result of the demonstration -- i.e., the difference in new business starts between the treatment and control groups. We also measured the net employment gains of nonparticipants that were attributable to the demonstration.

As in the case of psychological well-being, we did not attempt to quantify the monetary value of these demonstration impacts. If the net monetary benefits of the demonstrations are otherwise positive, then the addition of community economic effects will only enhance net benefits.

Earnings gains represent the main benefit that we were able to measure in monetary terms. In previous chapters, we isolated the separate demonstration impacts on self-employment earnings and on wage and salary earnings in order to gain insights into demonstration effects. For the cost-benefit analysis, however, the origin of the earnings gain is less important than the magnitude of the total gain. As a result, we use total earnings in the cost-benefit analysis.[108]

[108] Our measure of earnings gain does not include the value of fringe benefits. While it is relatively straight forward to impute fringe benefits for wage and salary earners, imputation of fringe benefits for the self-employed is more problematic. Inasmuch as self-employment earnings are an important component of the total earnings gain, we chose not to impute the value of fringe benefits.

In the cost-benefit analysis, we use the point estimate regardless of its statistical significance as the best estimate of program impacts. The rationale for this is as follows. In earlier chapters, when we were testing hypotheses about the program, impact estimates that were statistically insignificant resulted in inconclusive tests. In the benefit-cost analysis, however, we are not testing a hypothesis but rather we are trying to provide the best estimate of the net benefits of a particular program or service. In this environment, the actual point estimates represent the best measures of benefits and costs.

We use the Wave II impact estimates for the cost-benefit analysis for several reasons. First, since benefits are likely to accrue well after program services end, Wave I impacts may not fully capture the total program impacts. Indeed, for some participants, especially those in Massachusetts, Wave I results may reflect less than one year of experience after the end of program services. At the same time, extrapolating the results beyond the available observation period is dangerous -- especially in Massachusetts, where the economy and UI regulations changed dramatically during this period. As a result, we cannot be confident in extrapolating the impacts beyond the observation period using our current data.

It should be noted, however, that there is some evidence that program benefits may grow over time. For example, in Massachusetts, earnings gains in Wave II were greater than in Wave I. In Washington, we observed an increased impact on job creation in Wave II. Thus, truncating the observation period at less than three years after random assignment may understate long-term program benefits.

Based on this analysis and on data availability, we determined that the most appropriate time frame for the cost-benefit analysis is the longest observation period available in our data. Thus, we conclude that the Wave II impact estimates are the most appropriate estimates of earnings gains for the cost-benefit analysis.

As indicated in Chapter 7, the Wave II annualized impact of the Washington demonstration on total earnings was $289. The annualized Wave II impact of the Massachusetts

demonstration on total earnings (presented in Chapter 8) was $5,940.[109] In contrast to the Washington estimates, the Massachusetts estimates were statistically significant.

The measure of earnings that is appropriate for the benefit-cost analysis, however, is not annualized earnings, but rather, total earnings over the entire observation period. To derive total earnings over the entire observation period, we reestimated the earnings regressions with total earnings as the dependent variable.[110] The results of this reestimation of impacts on total earnings yields the following results.

Table 10.1

Demonstration Total Earnings Impacts

	Washington	Massachusetts
Wave II	$1,093	$14,859***

As indicated in Table 10.1, the Wave II (33-month) impact estimate in Washington is $1,093; the Massachusetts impact estimates is $14,859. The Massachusetts impact estimate is statistically significant.

Program Costs

The operational costs of the demonstration include the administrative costs of outreach, intake, and program management, and the costs of providing training and counseling to demonstration participants. We recognize that to accurately reflect the social cost of demonstration activities, the costs must include not only direct labor costs of demonstration staff, but also the associated fringe benefits and administrative overhead, and such nonlabor costs as

[109] The Massachusetts cost-benefit analysis is based on impact estimates derived from the Cohorts 1 and 2. Impact estimates derived from Cohort 3 are used later in this chapter to assess the sensitivity of the Massachusetts cost-benefit analysis results.

[110] As in previous chapters, fringe benefits are not included in the earnings figures and total earnings are deflated to 1990 dollars using a CPI index.

rent, utilities, and supplies. We did not, however, attempt to measure non-labor costs.[111] Instead, we focused our efforts on measuring the direct labor costs attributable to the demonstration. In addition, to appropriately measure the costs of an ongoing program, our measure of direct operational costs does *not* include several types of cost that are artifacts of the evaluation: the evaluation contractor's costs; any costs incurred by demonstration staff for evaluation data collection activities; and the costs of outreach and intake associated with the control group. We turn now to the measurement of program costs in each of the demonstrations.

Massachusetts Enterprise Project Costs

As discussed above, a number of costs are associated with an ongoing self-employment program. For example, there are administrative costs associated with operating an ongoing self-employment assistance program. An even larger component of total cost is the cost of providing training and counseling assistance to participants. Finally, there are the UI benefit payments that are paid to program participants.

Inasmuch as the Massachusetts Enterprise Project was an experimental demonstration, we use the operating costs of the demonstration only as a starting point and make adjustments to simulate the likely cost of an ongoing program. To simulate the administrative and service costs of an ongoing program, we eliminate those costs that are specific to the demonstration and would not be associated with an ongoing self-employment training program. For example, activities associated with random assignment and demonstration data entry are not included in the estimate of program costs.

Administrative Costs. Since our goal is to measure steady-state operating costs, rather than costs associated with program start-up, we collected administrative cost data late in the demonstration implementation (October 1992). October 1992 represents a period (during the third implementation phase) when program administrators and service providers had substantial experience with program operations and no new program adjustments were being implemented.

[111] Since we did not impute fringe benefits in the measurement of program benefits, we do not impute fringe benefits in the measurement of program costs.

For a four-week period during October 1992, each local office staff member involved with the demonstration was asked to complete a Staff Time Allocation Form (STAF). The form was designed to collect information on time spent conducting Information Sessions, conducting Enrollment Interviews, reviewing applications, discussions with central office staff, answering claimants' questions, and so forth. Similarly, a STAF was designed to collect information from the Enterprise Project Central Office Staff. The latter form collected information on program administrative activities. Time spent on administrative activities that are not associated with an ongoing program (e.g., preparing demonstration reports) was not included in our estimate of program costs.

To obtain administrative cost per participant, we divided the total cost of local office operations and Central Office operations (during this four-week period) by the average number of treatment group members served during a four-week period in phase three of the demonstration.

Using the above procedures, we derived an estimate of administrative cost per participant for the Massachusetts Enterprise Project. We estimated a cost of $56 for local office operations and a cost of $178 for Central Office operations. We now turn to the procedures used in deriving the costs of program services.

Service Costs. Estimating service costs in the Massachusetts Enterprise Project are relatively easy to estimate since all training and counseling services were provided by outside vendors. That is, after 1990, training and counseling services were provided by the vendors who were selected through competitive bidding. Thus, the service costs of the program during the second and third phases reflect the market cost of these services. Combining all vendor expenditures for Cohorts 2 and 3, and dividing this total by the number of treatment group members in Cohorts 2 and 3, we obtain an average cost per treatment group member of $782. This cost includes all training and counseling services provided to Massachusetts demonstration participants.

UI Payments. The results of our analysis of UI benefit payments (Chapter 8) indicate that the demonstration had a negative impact on these benefits during the first benefit year. That is, treatment group members collected less total benefits than control group members in the first benefit year. The magnitude of this reduction in UI benefits was -$758.

An analysis of UI benefits beyond the first benefit year indicated that savings continued to accrue to the UI Trust Fund beyond the first year. That is, in the second and third benefit years treatment group members continued to collect less UI benefits than control group members. The impact in the second benefit year was -$140 (insignificant). For the 31-month period following random assignment as a whole, the estimated impact of the demonstration on UI benefits was a reduction of $935.[112] We use the latter figure in the cost-benefit analysis.

Washington SEED Costs

The primary costs of operating the Washington SEED program on an ongoing basis are the costs of the business development specialists who provide the business assistance services, the costs of Job Service Center staff labor (for conducting the Information Sessions) and the costs of central office staff labor for performing project oversight and administrative duties (e.g., sending invitations for the Information Sessions, scheduling business training, reviewing program applications, administering the lump-sum payment process). There are also UI payments (including lump-sum payments) which, although a transfer and neutral from society's perspective overall, affect the distribution of benefits and costs between participants and nonparticipants. Below we outline our approach to measuring program costs for the Washington SEED project, and describe our cost estimates.

Administrative Costs. To measure the administrative costs of operating SEED, we relied on summary program cost records maintained in the central office, adjusted to cover only the functions required to operate the program on an ongoing basis. Because the research/evaluation and random assignment activities that would not be conducted as part of an ongoing program were exclusively performed by central office staff, we only had to make adjustments to the costs recorded for the central office. After making these adjustments, we estimate the central office staff administrative costs of operating a program like SEED on an ongoing basis to be $128 per participant. Our estimate of the cost per participant for local JSC staff is much less, $17. This is not surprising, given the limited role of JSC staff in the daily operations of SEED.

[112] To derive this impact estimate, we estimated a regression where the dependent variable was UI benefits collected during the 31-month period following random assignment.

Service Costs. Through an interagency agreement with the Department of Employment Security, the Business Assistance Center, a division of the Washington State Department of Trade and Economic Development, was responsible for recruiting and hiring Business Development Specialists (BDSs) who provided the business assistance services for the SEED Demonstration. Information on the costs incurred in providing SEED business assistance services was maintained by the Small Business Development Center (SBDC) of Washington State University. These costs included the salaries and fringe benefits paid to the BDSs who provided the business training workshops and counseling services to SEED treatment group members in the six sites. Combining all costs for business development specialist services across the six sites and dividing by the number of treatment group members results in an average cost per participant of $252.[113]

UI Payments. An important aspect of the distributional impacts of the program is the impact of SEED on UI payments. As described in Chapter 7, after including the lump-sum payment as part of total UI payments, we estimate that SEED increased total payments by +$1,098 during the first benefit year.

An analysis of UI benefit payments beyond the first benefit year indicated that there were very small differences between the treatment and control groups in benefit payments received during subsequent years. Specifically, the point estimate indicates that SEED reduced UI payments received after the initial benefit year by -$85, although the effect was not statistically significant. For the entire period following random assignment, the estimated impact of the SEED Demonstration on total benefit payments was +$1,013. This is the figure used in the cost-benefit analysis.

Comparison of Costs Between Massachusetts and Washington Programs

As the analysis described above indicates, the costs of the self-employment programs in Massachusetts and Washington differ considerably. The costs differ both in terms of the overall

[113] The estimated service costs of SEED are considerably lower than the corresponding service costs for the Massachusetts Enterprise Project. This may partly be due to the fact that in Washington some services (e.g., secretarial services, office space, and telephone) were made available through Community Colleges or other organizations.

amounts and in terms of their distribution. In Table 10.2, we provide summary information on the costs of the two programs.

Table 10.2
Comparison of Costs Between Massachusetts and Washington
(Dollars Per Claimant)

	Perspective		
	Participants	**Nonparticipants**	**Society**
Massachusetts			
UI Payments	-$935	+$935	0
Demonstration Costs			
Local Office Labor	0	-$56	-$56
Central Office Labor	0	-$178	-$178
Business Services	0	-$782	-$782
Total	**-$935**	**-$81**	**-$1,016**
Washington			
UI Payments	+$1,013	-$1,013	0
Demonstration Costs			
Local Office Labor	0	-$17	-$17
Central Office Labor	0	-$128	-$128
Business Services	0	-$252	-$252
Total	**+$1,013**	**-$1,410**	**-$397**

As shown in the final column, the costs of each component are much greater for the Massachusetts Enterprise Project than for SEED. The most striking difference concerns the costs of business services, for which we estimate a per participant cost of $782 for the Massachusetts program, as compared to only $252 for SEED. Although part of this differences may be due to differences in the underlying cost data available in the two states and the fact that the vendor cost data in Massachusetts may more closely reflect true resource costs, it is also likely that the large cost differences between the two States reflect the higher costs of providing more extensive business counseling services.

As described in Chapter 5, treatment group members in Washington received only 1.5 hours of counseling on average, as compared to 7.5 hours per person in Massachusetts. Also, the business training sessions in Washington were provided shortly after random assignment, whereas the business training workshops in Massachusetts were offered over a period of roughly 9-12 weeks. The large differences in the costs of business services per participant in the two programs in large part likely reflect the higher costs of providing more business counseling and training services over a longer period.

The results reported in Table 10.2 indicate that total social costs per participant in the Massachusetts program were slightly over $1,000. In contrast, per participant costs in Washington were just under $400, or less than one-half of the Massachusetts costs.

The distributional effects are also dramatically different. The overall cost per participant in Massachusetts of -$1,016 is comprised of -$935 from the perspective of participants and -$81 from nonparticipants. The preponderance of the costs falling onto participants is due to the reduction in UI payments they receive. In contrast, the overall cost per participant of -$397 in Washington can be decomposed into +$1,013 from the perspective of participants (due to the lump-sum payment), and -$1,410 from the perspective of nonparticipants.

Net Benefits and Costs

In this section, we present the net benefits and costs of the Massachusetts and Washington demonstrations. The estimates presented in this section are our best estimates of the benefits and costs of the two demonstrations. In the next section of this chapter, we present a number of

alternative estimates of net benefits and costs to assess the sensitivity of our estimate to alternative assumptions.

From all the perspectives examined (i.e., participants, nonparticipants, and society), the Massachusetts demonstration had positive net benefits. The Washington demonstration, on the other hand, had positive net benefits only from the perspective of participants and from the perspective of society as a whole. From the perspective of nonparticipants, the Washington demonstration had negative net benefits. We review each of these results below.

Massachusetts

Table 10.3 presents the estimated benefits and costs in Massachusetts. As indicated above (Table 10.1), in Massachusetts participants experienced a $14,859 increase in total earnings during the 31-month observation period relative to the control group. This estimate of earning gains does not include the value of fringe benefits that may be associated with increased earnings.[114] We have chosen to present earnings without this imputation for fringe benefits since imputing fringe benefits, especially for the self-employed, is subject to a great deal of uncertainty. Our estimates of earnings gains may, thus, be viewed as conservative estimates of the actual gain.

Over the same period, participants paid $2,229 in additional taxes[115] and received $876 less in UI benefits than their control group counterparts. Summing all these gains and losses, the net benefit was $11,754 from the perspective of the average participant.

The increased taxes paid by participants ($2,229) as well as the reduced UI benefits received by participants ($876) are both benefits to nonparticipants. The cost of providing self-employment training services, however, represent a cost to nonparticipants. Summing the benefits and costs to nonparticipants, yields a net benefit of $2,089.

For society as a whole, the principal benefit of the program is the earnings gain. The principal cost, on the other hand, is the cost of implementing the program. Given the substantial earnings gain and the relatively modest program cost, the net benefit to society is $13,843.

---

[114] Studies of employee fringe benefits by the U.S. Chamber of Commerce and others have estimated that the value of these benefits are approximately 25 percent of total wage and salary outcomes.

[115] Increased taxes on earnings are estimated as 15 percent of earnings gains.

Table 10.3

Massachusetts
Benefit-Cost Analysis
(Dollars per Claimant)

Benefits and Costs	Perspective		
	Participants	Nonparticipants	Society
Earnings	$14,859	$0	$14,859
Taxes	-2,229	+2,229	0
UI Payments	-876	+876	0
Demonstration Costs			
Local Office Labor	0	-56	-56
Central Office Labor	0	-178	-178
Services	0	-782	-782
NET BENEFITS	$11,754	$2,089	$13,843

In Table 10.4, we assess the benefits and costs of the Massachusetts Enterprise Project from the government's perspective. We decompose total government into the Labor Department and all other government agencies.[116] As indicated in Table 10.4, the Massachusetts Enterprise Project had positive net benefits from total government's perspective as well as from DOL's perspective from other agencies' perspective. It should be noted, however, that the positive net benefit to DOL is quite small ($16).[117] In contrast, the net benefit to total government is quite substantial ($2,089), resulting largely from the increased taxes generated

[116] In our analysis we do not isolate the benefits and costs to the UI Trust Fund; rather, we incorporate these benefits and costs into the Labor Department perspective.

[117] The small positive net benefit to DOL is subject to a great deal of uncertainty and could easily have been negative under slightly different assumptions. For example, it is necessary to apportion the additional tax revenue between the Department of Labor and the rest of government. We used 1.05 percent of total wage and salary income as the portion of tax payments which go to the UI system. This average rate for the U.S. was reported in USDOL's *Handbook of Unemployment Insurance Financial Data.*

Table 10.4

Massachusetts
Benefit-Cost Analysis

Government Perspective
(Dollars per Claimant)

Benefits and Costs	Perspective		
	Labor Department	Other Government	Total Government
Earnings	$0	$0	$0
Taxes	+156	+2,073	+2,229
UI Payments	+876	0	+876
Demonstration Costs			
Local Office Labor	-56	0	-56
Central Office Labor	-178	0	-178
Services	-782	0	-782
NET BENEFITS	+$16	$2,073	$2,089

by the higher earnings of the treatment group members relative to the control group members.

In conclusion, the Massachusetts Enterprise Project generates net benefits from every perspective examined, including the government perspective. Given these strong results, the Massachusetts Enterprise Project appears to be a highly cost-effective policy tool for assisting UI claimants who are interested in pursuing self-employment.

Washington

The estimated benefits and costs in Washington are presented in Table 10.5. In Washington, participants experienced an increase of $1,093 in earnings during the 33-month observation period relative to the control group (see Table 10.1). As a result of this increase

Table 10.5

Washington
Benefit-Cost Analysis
(Dollars per Claimant)

Benefits and Costs	Perspective		
	Participants	Nonparticipants	Society
Earnings	$1,093	$0	$1,093
Taxes	-164	164	0
UI Payments	1,013	-1,013	0
Demonstration Costs			
Local Office Labor	0	-17	-17
Central Office Labor	0	-128	-128
Services	0	-252	-252
NET BENEFITS	$1,942	-$1,246	$696

in earnings, participants paid $164 in additional taxes.[118] In addition to their higher earnings, participants also received higher UI benefits (including the lump-sum payment) than their control group counterparts ($1,013).

Summing all the gains and losses for participants we find that from the participants' perspective, the net benefit of SEED is $1,942 per participant. Summing all the gains and losses for nonparticipants, on the other hand, reveals that costs exceed benefits, yielding a net cost of -$1,246 per participant. Finally, summing the gains and losses for society as a whole yields a net benefit of $696 per participant.

In Table 10.6, we present the benefits and costs from the government's perspective. As we did above for Massachusetts, we decompose total government into the Labor Department and all other government agencies. The results reported in Table 10.6 indicate that the Washington SEED program had a net cost to the Labor Department (-$1,399) and a net cost to the government as a whole (-$1,246). Other government agencies, however, experienced a net

[118] Increased taxes on earnings are estimated as 15 percent of earnings gains.

benefit of $153 from the additional taxes generated by the increased earnings of treatment group members relative to control group members.

In conclusion, the Washington SEED program had positive net benefits from the participants' perspective and from society's perspective; it had net costs from the nonparticipants' perspective and from government's perspective. Thus, the Washington

Table 10.6

Washington
Benefit-Cost Analysis

Government Perspective
(Dollars per Claimant)

Benefits and Costs	Perspective		
	Labor Department	Other Government	Total Government
Earnings	$0	$0	$0
Taxes	+11	+153	+164
UI Payments	-1,013	0	-1,013
Demonstration Costs			
Local Office Labor	-17	0	-17
Central Office Labor	-128	0	-128
Services	-252	0	-252
NET BENEFITS	-$1,399	$153	-$1,246

program, like the Massachusetts program, is cost-effective from society's perspective. However, in contrast to the Massachusetts program, the Washington program has significant redistributional implications. Approximately two-thirds of the net benefits enjoyed by participants reflect direct costs to nonparticipants.

SENSITIVITY OF THE NET BENEFIT ESTIMATES

The cost-benefit results presented in previous sections are based on benefit and cost estimates that are subject to some uncertainty. In addition, a number of assumptions were necessary to derive the program impact estimates used in these cost-benefit calculations. For example, in estimating program impacts in Washington, we made a decision to include an outlier who dramatically affected the impact results. Moreover, in Massachusetts, we did not use the Cohort 3 results in the main impact analysis. Below we examine the sensitivity of the benefit-cost results to alternative assumptions on these issues.

Excluding an Outlier in Washington

As indicated in Chapter 7, the main SEED impact results included one treatment group member who reported self-employment earnings that were dramatically higher than the earnings of all other sample members (more than $500,000 over the Wave I observation period). Because a comparison of the survey data with administrative records available in Washington State could not convincingly demonstrate the survey data were incorrect, we decided to include the claimant in the impact analysis. We noted, however, that the annualized program impact estimates are reduced by some $400-$500 when this claimant is excluded.

To determine the sensitivity of the overall benefit-cost results to the inclusion of this case, we excluded this case and reestimated the impacts on total earnings from wage and salary employment and self-employment over the 33 month observation period.[119] After excluding this case, the estimated impact of SEED on total earnings over the observation period is -$68, which is not statistically significantly different from zero. As shown in table 10.7, the benefit-cost results change dramatically when this claimant is excluded from the analysis. Specifically, the estimated benefits from the participants' perspective are reduced from $1,942 to $955; the costs from the perspective of nonparticipants increase from -$1,246 to -$1,420; and the overall

[119] An alternative approach would have been to include the claimant, but at some intermediate earnings value. However, the ESD data were insufficient to allow us to develop an alternative estimate of self-employment earnings for this case.

Table 10.7

Washington
Benefit-Cost Analysis

Excluding Outlier
(Dollars per Claimant)

Benefits and Costs	Perspective		
	Participants	Nonparticipants	Society
Earnings	-$68	$0	-$68
Taxes	+10	-10	0
UI Payments	1,013	-1,013	0
Demonstration Costs			
Local Office Labor	0	-17	-17
Central Office Labor	0	-128	-128
Services	0	-252	-252
NET BENEFITS	$955	-$1,420	-$465

net benefits per participant from society's perspective change from $696 to -$465. We view this as the lower bound estimate for SEED.

These results indicate that excluding this claimant alters the conclusions on the cost-effectiveness of SEED. Without this individual, the Washington SEED program can no longer be viewed as a cost-effective program from society's perspective overall.[120] For SEED to be viewed as cost-effective from society's perspective, the monetary value of the other benefits that are not measured (e.g., improved psychological well-being, community economic development) would have to exceed $465 per participant.

[120] It is interesting to note that excluding this case - and using an estimated impact on total earnings of -$68 - is very similar to using an impact estimate of $0, which is the value that would be used under the alternative procedure of using 0 when the estimated impacts are not significantly different from zero.

Using Cohort 3 Impact Estimates in Massachusetts

As described in Chapter 4, a number of important changes occurred in the UI system at the time that Cohort 3 was being enrolled in the Massachusetts Enterprise Project. To assess the sensitivity of the Massachusetts cost-benefit results to changes in operating environment, we use the impact estimates derived from Cohort 3 in the cost-benefit analysis. As discussed in Chapter 8, we estimated program impacts separately for those who filed a UI claim before June 1, 1992 and those who filed after that date. In Tables 10.8 and 10.9, we present the benefit-cost results using the impact estimates from the two Cohort 3 subgroups.[121]

As indicated in Table 10.8, the impact of the Massachusetts Enterprise Project on total earnings for this group was negative (-$540). Moreover, participants experienced a negligible reduction in UI payments. Summing the benefits and costs yields a net cost of $465 from the perspective of participants. Furthermore, as indicated in Table 10.8, there were also net costs from the perspective of nonparticipants and society as a whole.

[121] Given the relatively short observation period available for Cohort 3 members, we use the annualized earning impacts. We also use the UI impacts in the first benefit year in these cost-benefit calculations.

Table 10.8

Massachusetts
Benefit-Cost Analysis

Cohort 3: Prior to June 1, 1992
(Dollars per Claimant)

Benefits and Costs	Perspective		
	Participants	Nonparticipants	Society
Earnings	-$540	$0	-$540
Taxes	+81	-81	0
UI Payments	-6	+6	0
Demonstration Costs			
Local Office Labor	0	-56	-56
Central Office Labor	0	-178	-178
Services	0	-782	-782
NET BENEFITS	-$465	-$1,091	-$1,556

Table 10.9

Massachusetts
Benefit-Cost Analysis

Cohort 3: After June 1, 1992
(Dollars per Claimant)

Benefits and Costs	Perspective		
	Participants	Nonparticipants	Society
Earnings	+$1,371	$0	+$1,371
Taxes	-206	+206	0
UI Payments	+388	-388	0
Demonstration Costs			
Local Office Labor	0	-56	-56
Central Office Labor	0	-178	-178
Services	0	-782	-782
NET BENEFITS	+$1,553	-$810	+$355

Appendix A

Definition of Independent Variables Included in Regression Models

Definition of Independent Variables Included in Regression Models: Washington SEED Demonstration	
Independent Variables	
Treatment Group Dummy	1 if claimant assigned to treatment group; 0 otherwise
Vancouver Dummy	1 if UI claim filed in Vancouver; 0 otherwise
King County Dummy	1 if UI claim filed in King County; 0 otherwise
Snohomish County Dummy	1 if UI claim filed in Snohomish County; 0 otherwise
Wenatchee Dummy	1 if UI claim filed in Wenatchee; 0 otherwise
Yakima Dummy	1 if UI claim filed in Yakima; 0 otherwise
Benefit Year Started in Quarter 1 of 1990	1 if claimant's benefit year started in Quarter 1 of 1990; 0 otherwise
Benefit Year Started in Quarter 2 of 1990	1 if claimant's benefit year started in Quarter 2 of 1990; 0 otherwise
Benefit Year Started in Quarter 4 of 1989	1 if claimant's benefit year started in Quarter 4 of 1990; 0 otherwise
Age (in years)	age in years
Age Squared	age^2
Male Dummy	1 if male; 0 otherwise
White Dummy	1 if white; 0 otherwise
Completed College Dummy	1 if highest grade completed is 16 or more; 0 otherwise

Definition of Independent Variables Included in Regression Models: Washington SEED Demonstration	
Independent Variables	
Prior Job in Professional, Technical or Managerial Profession Dummy	1 if prior job in profession; 0 otherwise
Prior Job in Services Sector Dummy	1 if prior job was in Service Sector; 0 otherwise
Intended to return to work to prior employer	1 if claimant indicated on SEED application she/he intended to return to work to prior employer; 0 otherwise
Spouse Employed Dummy	1 if spouse employed; 0 otherwise
Children under age six Dummy	1 if claimant has children under the age of six; 0 otherwise
Prior work experience in the area of the proposed business dummy	1 if claimant has prior work experience in the area of the proposed business; 0 otherwise
Had a business at the time of SEED application dummy	1 if claimant reported having a business at the time of SEED application; 0 otherwise
Self-employed prior to (but not at the time) of SEED application dummy	1 if claimant was self-employed prior to SEED application but not at time of SEED application; 0 otherwise
High wage earner in the four complete quarters before filing the UI claim dummy	1 if claimant reported being in upper earnings quartile in the four complete quarters before filing UI claim; 0 otherwise
Medium wage earner in the four complete quarters before filing the UI claim dummy	1 if claimant reported being in two middle quartiles in the four complete quarters before filing UI claim; 0 otherwise
Unemployment Rate in the claimant's county of residence during 1990	Unemployment Rate in the claimant's county of residence during 1990

Definition of Independent Variables Included in Regression Models: Massachusetts Enterprise Demonstration

Independent Variables	
Treatment Group Dummy	1 if claimant assigned to treatment group; 0 otherwise
Gloucester Dummy (Cohort 1 only)	1 if UI claim filed in Gloucester; 0 otherwise
Greenfield Dummy	1 if UI claim filed in Greenfield; 0 otherwise
Lowell Dummy	1 if UI claim filed in Lowell; 0 otherwise
Milford Dummy (Cohorts 2 and 3 only)	1 if UI claim filed in Milford; 0 otherwise
New Bedford Dummy (Cohorts 1 and 2 only)	1 if UI claim filed in New Bedford; 0 otherwise
Northampton Dummy (Cohort 3 only)	1 if UI claim filed in Northampton; 0 otherwise
Roxbury County Dummy	1 if UI claim filed in Roxbury County; 0 otherwise
Springfield Dummy	1 if UI claim filed in Springfield; 0 otherwise
Woburn Dummy	1 if UI claim filed in Woburn; 0 otherwise
Benefit Year Started in Quarter 1 of 1990	1 if claimant's benefit year started in Quarter 1 of 1990; 0 otherwise
Benefit Year Started in Quarter 2 of 1990	1 if claimant's benefit year started in Quarter 2 of 1990; 0 otherwise
Benefit Year Started in Quarter 4 of 1989	1 if claimant's benefit year started in Quarter 4 of 1990; 0 otherwise
Age (in years)	age in years
Age Squared	age^2
Male Dummy	1 if male; 0 otherwise
White Dummy	1 if white; 0 otherwise
Completed College Dummy	1 if highest grade completed is 16 or more; 0 otherwise

Definition of Independent Variables Included in Regression Models: Massachusetts Enterprise Demonstration	
Independent Variables	
Prior Job in Professional, Technical or Managerial Profession Dummy	1 if prior job in profession; 0 otherwise
Prior Job in Services Sector Dummy	1 if prior job was in Service Sector; 0 otherwise
Intended to return to work to prior employer	1 if claimant indicated on SEED application she/he intended to return to work to prior employer; 0 otherwise
Children under age six Dummy	1 if claimant has children under the age of six; 0 otherwise
Prior work experience in the area of the proposed business dummy	1 if claimant has prior work experience in the area of the proposed business; 0 otherwise
High wage earner in the four complete quarters before filing the UI claim dummy	1 if claimant reported being in upper earnings quartile in the four complete quarters before filing UI claim; 0 otherwise
Medium wage earner in the four complete quarters before filing the UI claim dummy	1 if claimant reported being in two middle quartiles in the four complete quarters before filing UI claim; 0 otherwise
Unemployment Rate in the claimant's local office catchment area during 1990, 1991, 1992, and 1993	Unemployment Rate in the claimant's local office catchment area in 1990, 1991, 1992, and 1993

Appendix B

Significant Coefficients

Notes for Appendix B

In this appendix the definition of the column headings **A**, **B**, **C** and **D** vary according to whether the table describes self-employment outcomes, wage and salary outcomes, or combined self-employment and wage and salary employment outcomes.

Self-Employment Outcomes

A =whether the person experienced at least one spell of self-employment between random assignment and the date of the follow-up survey.

B =annualized time spent in self-employment, between random assignment and the date of the follow-up survey.

C =annualized real earnings (in 1990 dollars) from self-employment, between random assignment and the date of the follow-up survey.

D =whether the person was in self-employment at the time of the follow-up survey.

Wage and Salary Employment Outcomes

A =whether the person experienced at least one spell of wage and salary employment between random assignment and the date of the follow-up survey.

B =annualized time spent in wage and salary employment, between random assignment and the date of the follow-up survey.

C =annualized real earnings (in 1990 dollars) from wage and salary employment, between random assignment and the date of the follow-up survey.

D =whether the person was in wage and salary employment at the time of the follow-up survey.

Combined Self-Employment and Wage and Salary Employment Outcomes

A =whether the person experienced at least one spell of either self-employment or wage and salary employment between random assignment and the date of the follow-up survey.

B =annualized time spent in any type of employment, regardless of whether it was self-employment or wage and salary employment, between random assignment and the date of the follow-up survey.

C =annualized real earnings (in 1990 dollars) from any type of employment, between random assignment and the date of the follow-up survey.

D =whether the person was in either self-employment or wage and salary employment at the time of the follow-up survey.

Significant Coefficients

Washington Wave I

Variables	Self-Employment				Wage and Salary Job				Combined SE and WS			
	A	B	C	D	A	B	C	D	A	B	C	D
Treatment/Control Group	+	+	+	+	–	–	–	–	+	+		
White	+				+	+	+				+	
Male		–				–				–		
Age						+				+		
Age Squared						–				–		
College Graduate	+		+				+				+	
Children Under 6												
Current Business Owner	+			+	-							
Related Work Experience	+	+		+	–			–				
Expect to be Recalled		–			+	–				–		
Had a Business Before												
Prior Earnings Top Quartile	+	+	+	+	–		+		+	+	+	+
Prior Earnings Middle Quartiles	+	+		+		+	+	+	+	+	+	+
Spouse Working	+	+		+	–							
Prof/Tech/Mgr Occupation						+						+
Services Industry									–	–		
Site: King County										–		
Site: Snohomish County												
Site: Vancouver	+										–	
Site: Wenatchee									+			
Site: Yakima								+	+	+		
Benefits Started 1990:I												

Variables	Self-Employment				Wage and Salary Job				Combined SE and WS			
	A	B	C	D	A	B	C	D	A	B	C	D
Benefits Started 1990:II				–								
Benefits Started 1990:IV	–	–		–	+	+	+				+	
1990 Site Unemployment Rate	+			+								

Washington Wave II

Variables	Self-Employment				Wage and Salary Job				Combined SE and WS			
	A	B	C	D	A	B	C	D	A	B	C	D
Treatment/Control Group	+	+	+	+	–	–	–		+	+		+
White	+			+						+		
Male				+		–					+	+
Age						+	+	+		+		+
Age Squared				–		–	–			–	–	–
College Graduate	+		+				+				+	+
Children Under 6								–				
Current Business Owner	+			+								+
Related Work Experience	+	+		+	–	–		–				
Expect to be Recalled					+			+		–		
Had a Business Before							–					
Prior Earnings Top Quartile	+	+	+	+	–		+			+	+	+
Prior Earnings Middle Quartiles		+				+	+			+	+	+
Spouse Working	+	+		+				–				
Prof/Tech/Mgr Occupation											+	
Services Industry									–	–		
Site: King County								–				
Site: Snohomish County												
Site: Vancouver												
Site: Wenatchee	–											
Site: Yakima								+				+

Variables	Self-Employment				Wage and Salary Job				Combined SE and WS			
	A	B	C	D	A	B	C	D	A	B	C	D
Benefits Started 1990:I			+			+		+			+	
Benefits Started 1990:II					+			+				
Benefits Started 1990:IV	–	–			+	+	+				+	
1990 Site Unemployment Rate								–				

Massachusetts Cohorts 1 and 2, Wave I

Significant Coefficients

Variables	Self-Employment				Wage and Salary Job				Combined SE and WS			
	A	B	C	D	A	B	C	D	A	B	C	D
Treatment/Control Group	+	+		+		+	+		+	+	+	+
White												
Male	+			+		−						
Age					−	−			−	−		
Age Squared						+						
College Graduate							+					
Expect to be Recalled												
Prior Earnings Top Quartile												+
Prior Earnings Middle Quartiles								+				+
Prof/Tech/Mgr Occupation		+		+								
Services Industry	−	−		−	+	+	+	+				
Site: Gloucester												
Site: New Bedford												
Site: Springfield						+	+			+		
Site: Roxbury	−			−								
Site: Milford					+	+	+	+		+		
Site: Lowell												
Site: Greenfield			+								+	
Benefits Started 1990:II					+							
Benefits Started 1990:III											+	
Benefits Started 1991:I		−			+	+	+	+				
Benefits Started 1991:II					+		+	+				
1990 Site Unemployment Rate							−					

Massachusetts Cohorts 1 and 2, Wave II

Significant Coefficients

Variables	Self-Employment				Wage and Salary Job				Combined SE and WS			
	A	B	C	D	A	B	C	D	A	B	C	D
Treatment/Control Group	+	+					+		+	+	+	+
White						+						
Male	+		+			–						
Age												
Age Squared												
College Graduate							+				+	
Expect to be Recalled												–
Prior Earnings Top Quartile							+					
Prior Earnings Middle Quartiles												
Prof/Tech/Mgr Occupation	+											
Services Industry	–	–		–	+	+		+				
Site: Gloucester												
Site: New Bedford		–										
Site: Springfield	+						+			+	+	+
Site: Roxbury	–	–										
Site: Milford					+		+					
Site: Lowell												
Site: Greenfield												+
Benefits Started 1990:II		–										
Benefits Started 1990:III												
Benefits Started 1991:I	–	–		–	+	+	+	+				
Benefits Started 1991:II						+	+	+			+	
1990 Site Unemployment Rate				+		–	–	–			–	

Appendix C

Effects on Subgroups

Notes for Appendix C

This appendix defines the column headings **A**, **B**, **C** and **D** which appear in some of the impacts tables. The definition for each letter varies according to whether the table describes self-employment outcomes, wage and salary outcomes, or combined self-employment and wage and salary employment outcomes.

Self-Employment Outcomes

A =whether the person experienced at least one spell of self-employment between random assignment and the date of the follow-up survey.

B =annualized time spent in self-employment, between random assignment and the date of the follow-up survey.

C =annualized real earnings (in 1990 dollars) from self-employment, between random assignment and the date of the follow-up survey.

D =whether the person was in self-employment at the time of the follow-up survey.

Wage and Salary Employment Outcomes

A =whether the person experienced at least one spell of wage and salary employment between random assignment and the date of the follow-up survey.

B =annualized time spent in wage and salary employment, between random assignment and the date of the follow-up survey.

C =annualized real earnings (in 1990 dollars) from wage and salary employment, between random assignment and the date of the follow-up survey.

D =whether the person was in wage and salary employment at the time of the follow-up survey.

Combined Self-Employment and Wage and Salary Employment Outcomes

A =whether the person experienced at least one spell of either self-employment or wage and salary employment between random assignment and the date of the follow-up survey.

B =annualized time spent in any type of employment, regardless of whether it was self-employment or wage and salary employment, between random assignment and the date of the follow-up survey.

C =annualized real earnings (in 1990 dollars) from any type of employment, between random assignment and the date of the follow-up survey.

D =whether the person was in either self-employment or wage and salary employment at the time of the follow-up survey.

Subgroup Effects

Washington Wave I

Variables	Self-Employment				Wage and Salary Job				Combined SE and WS			
	A	B	C	D	A	B	C	D	A	B	C	D
White									+			
Male	–	–									+	
Expect to be Recalled		–							–		+	–
Prior Earnings in Top Quartile compared to Bottom Quartile			+		–	–						
Prior Earnings in Middle Two Quartiles compared to Bottom Quartile												
Prior Earnings in Top Quartile compared to Middle Two Quartiles			+									

Washington Wave II

Variables	Self-Employment				Wage and Salary Job				Combined SE and WS			
	A	B	C	D	A	B	C	D	A	B	C	D
White									+			
Male	–										+	
Expect to be Recalled											+	
Prior Earnings in Top Quartile compared to Bottom Quartile			+									
Prior Earnings in Middle Two Quartiles compared to Bottom Quartile												
Prior Earnings in Top Quartile compared to Middle Two Quartiles			+									

Subgroup Effects

Massachusetts Cohorts 1 and 2, Wave I

Variables	Self- Employment				Wage and Salary Job				Combined SE and WS			
	A	B	C	D	A	B	C	D	A	B	C	D
White		+			–				–			
Male					–							
Expect to be Recalled												
Prior Earnings in Top Quartile compared to Bottom Quartile						+	+					
Prior Earnings in Middle Two Quartiles compared to Bottom Quartile		–	–									
Prior Earnings in Top Quartile compared to Middle Two Quartiles												
Cohort 1												

Massachusetts Cohorts 1 and 2, Wave II

Variables	Self-Employment				Wage and Salary Job				Combined SE and WS			
	A	B	C	D	A	B	C	D	A	B	C	D
White												
Male					–							
Expect to be Recalled												
Prior Earnings in Top Quartile compared to Bottom Quartile												
Prior Earnings in Middle Two Quartiles compared to Bottom Quartile												
Prior Earnings in Top Quartile compared to Middle Two Quartiles												
Cohort 1												

UI OCCASIONAL PAPER SERIES

The Unemployment Insurance Occasional Paper Series presents research findings and analyses dealing with unemployment insurance issues. Papers are prepared by research contractors, staff members of the unemployment insurance system, or individual researchers. Manuscripts and comments from interested individuals are welcomed. All correspondence should be sent to:

UI Occasional Paper Series
U.S. Department of Labor
Unemployment Insurance Service
Employment and Training Administration
200 Constitution Ave., NW Room S-4519
Washington, D.C. 20210
Attention: Rosalind Thomas

Arrangements have been made for the sale of most of the reports in the series through a Federal information and retrieval system, the National Technical Information Service (NTIS). Copies of the reports are available from NTIS in paper or microfiche. The NTIS accession number for the paper copy is listed after the title of each paper. To obtain the papers from NTIS, forward orders to:

National Technical Information Service
U.S. Department of Commerce
5285 Port Royal Road
Springfield, Virginia 22161
Telephone: (703) 487-4600

Papers which are not available are indicated with an asterisk.

TITLE	UIOP #
1977	
G. Joachim Elterich and Linda Graham, *Impact of Extension of Coverage to Agricultural Workers Under P.L. 94-566, Their Characteristics and Economic Welfare*, University of Delaware. NTIS PB83-147819.	77-1

G. Joachim Elterich and Linda Graham, Impact of P.L. 94-566 on Agricultural Employers and Unemployment Insurance Trust Funds in Selected States, University of Delaware. NTIS PB83-147827. 77-2

David Stevens, Unemployment Insurance Beneficiary Job Search Behavior: What Is Known and What Should Be Known for Administrative Planning Purposes, University of Missouri. 77-3

Michael Klausner, Unemployment Insurance and the Work Disincentive Effect: An Examination of Recent Research, Unemployment Insurance Service. 77-4

Gary Solon, Weekly Benefit Amounts and Normal Weekly Wages of Unemployment Insurance Claimants, Unemployment Insurance Service. 77-5

Ruth Entes, Family Support and Expenditures Survey of Unemployment Insurance Claimants in New York State, September 1972-February 1974, New York State Department of Labor. 77-6

Saul Blaustein and Paul Mackin Development of the Weekly Benefit Amount in Unemployment Insurance, Upjohn Institute. 77-7

Saul Blaustein and Paul Mackin, Job Loss, Family Living Standards, and the Adequacy of Weekly Unemployment Benefits, Upjohn Institute 77-8

1978

Henry Felder and Richard West, The Federal Supplemental Benefits Program: National Experience and the Impact of P.L. 95-19, SRI International. NTIS PB83-149633. 78-1

Paul Burgess, Jerry Kingston and Chris Walters, The Adequacy of Unemployment Insurance Benefits: An Analysis of Weekly Benefits Relative to Preunemployment Expenditure Levels, Arizona Department of Economic Security and Arizona State University. NTIS PB83-148528. 78-2

Christopher Pleatsikas, Lawrence Bailis and Judith Dernburg, A Study of Measures of Substantial Attachment to the Labor Force, Volumes I and II, Urban Systems Research and Engineering, Inc. Vol I: NTIS PB83-147561. Vol. II: NTIS PB83-147579. 78-3

Henry Felder and Randall Pozdena, The Federal Supplemental Benefits Program: Impact of P.L. 95-19 on Individual Recipients, SRI International. NTIS PB83-149179. 78-4

Peter Kauffman, Margaret Kauffman, Michael Werner and Christine Jennison, An Analysis of Some of the Effects of Increasing the Duration of Regular Unemployment Insurance Benefits, Management Engineers, Inc. 78-5

Jerry Kingston, Paul Burgess and Chris Walters, The Adequacy of Unemployment Insurance Benefits: An Analysis of Adjustments Undertaken Through Thirteen and Twenty-Five Weeks of Unemployment, Arizona Department of Economic Security and Arizona State University. NTIS PB83-149823. 78-6

Walter Nicholson and Walter Corson, The Effect of State Laws and Economic Factors on Exhaustion Rates for Regular Unemployment Insurance Benefits: A Statistical Model, Mathematica Policy Research. NTIS PB83-149468. 78-7

Louis Benenson, Incidence of Federal Retirees Drawing UCFE Benefits, 1974-75, Unemployment Insurance Service. NTIS PB83-161927. 78-8

1979

Henry Felder, A Statistical Evaluation of the Impact of Disqualification Provisions of State Unemployment Insurance Laws. SRI International. NTIS PB83-152272. 79-1

Arthur Denzau, Ronald Oaxaca and Carol Taylor, The Impact of Unemployment Insurance Benefits on Local Economies--Tucson, University of Arizona. NTIS PB83-169912. 79-2

Paul Burgess, Jerry Kingston and the Research and Reports Section of the Unemployment Insurance Bureau, Arizona Department of Economic Security, Labor Market Experiences of Unemployment Insurance Exhaustees, Arizona Department of Economic Security and Arizona State University. NTIS PB83-224162. 79-3

Carolyn Sperber, An Evaluation of Current and Alternative Methods of Determining Exhaustion Ratios, Unemployment Insurance Service. NTIS PB83-148866. 79-4

Mamoru Ishikawa, Unemployment Compensation in Varying Phases of Joblessness, Unemployment Insurance Service. NTIS PB83-150581. 79-5

Nicholas Kiefer and George Neumann, The Effect of Alternative Partial Benefit Formulas on Beneficiary Part-Time Work Behavior, National Opinion Research Center. NTIS PB83-146811. 79-6

1980

Mamoru Iskikawa, Unemployment Insurance and Proliferation of Other Income Protection Programs for Experienced Workers, Unemployment Insurance Service. NTIS PB83-140657. 80-1

UI Research Exchange. Information on unemployment insurance research. First issue: 1980, Unemployment Insurance Service. NTIS PB83-148411. 80-2

Raymond P.F. Fishe and G.S. Maddala, Effect of Unemployment Insurance on Duration of Unemployment: A Study Based on CWBH Data for Florida, Florida State University and University of Florida. PB88-162464. 80-3

Jerry Kingston, Paul Burgess, Robert St. Louis and Joseph Sloane, Benefit Adequacy and UI Program Costs: Simulations with Alternative Weekly Benefit Formulas, Arizona Department of Economic Security and Arizona State University. 80-4

1981

UI Research Exchange. Information on unemployment insurance research. First issue: 1981. Unemployment Insurance Service. NTIS PB83-152587. 81-1

Jerry Kingston, Paul Burgess, Robert St. Louis and Joseph Sloane, Can Benefit Adequacy Be Predicted on the Basis of UI Claims and CWBH Data? Arizona Department of Economic Security and Arizona State University. NTIS PB83-140566. 81-2

Paul Burgess, Jerry Kingston, Robert St. Louis and Joseph Sloane, Changes in Spending Patterns Following Unemployment, Arizona Department of Economic Security and Arizona State University. NTIS PB83-148833. 81-3

UI Research Exchange. Information on unemployment insurance research. Second issue: 1981, Unemployment Insurance Service. NTIS PB83-148429. 81-4

1983

Walter Corson and Walter Nicholson, An Analysis of UI Recipients' Unemployment Spells, Mathematica Policy Research. 83-1
NTIS PB84-151463.

Lois Blanchard and Walter Corson, A Guide to the Analysis of UI Recipients' Unemployment Spells Using a Supplemented CWBH Data Set, Mathematica Policy Research. 83-2
NTIS PB84-151471.

Ronald L. Oaxaca and Carol A. Taylor, The Effects of Aggregate Unemployment Insurance Benefits in the U.S. on the Operation of a Local Economy, University of Arizona. 83-3
NTIS PB84-150317.

UI Research Exchange. Information on unemployment insurance research. 1983 issue. Unemployment Insurance Service. 83-4
NTIS PB84-150325.

1984

UI Research Exchange. Information on unemployment insurance research. 1984 issue. Unemployment Insurance Service. 84-1
NTIS PB85-180370.

Stephen Wandner, John Robinson and Helen Manheimer Unemployment Insurance Schemes in Developing Countries, Unemployment Insurance Service. 84-2
NTIS PB85-185098/AS.

1985

Walter Corson and Walter Nicholson, An Analysis of the 1981-82 Changes in the Extended Benefit Program, Mathematica Policy Research. 85-1
NTIS PB85-176287/AS.

Walter Corson, David Long and Walter Nicholson, Evaluation of the Charleston Claimant Placement and Work Test Demonstration, Mathematica Policy Research. 85-2
NTIS PB85-152965.

Walter Corson, Alan Hershey, Stuart Kerachsky, Paul Rynders and John Wichita, Application of the Unemployment Insurance System Work Test and Nonmonetary Eligibility Standards, Mathematica Policy Research. 85-3
NTIS PB85-169910/AS.

Robert Moffitt, The Effect of the Duration of Unemployment Benefits on Work Incentives: An Analysis of Four Data Sets, Mathematica Policy Research. 85-4
NTIS PB85-170546.

Helen Manheimer and Evangeline Cooper, Beginning the Unemployment Insurance Program--An Oral History, Unemployment Insurance Service. 85-5
NTIS PB87-117370/AS.

1986

Helen Manheimer, John Robinson, Norman Harvey, William Sheehan and Burman Skrable, Alternative Uses of Unemployment Insurance, Unemployment Insurance Service. 86-1
NTIS PB87-118402/AS.

Norman Harvey, Unemployment Insurance Bibliography, Unemployment Insurance Service. 86-2
NTIS PB87-118410/AS.

Walter Corson, Jean Grossman and Walter Nicholson, An Evaluation of the Federal Supplemental Compensation Program, Mathematica Policy Research. 86-3
NTIS PB86-163144.

Stuart Kerachsky, Walter Nicholson and Alan Hershey, An Evaluation of Short-Time Compensation Programs, Mathematica Policy Research. 86-4
NTIS PB86-167616.

James M. Rosbrow, Fifty Years of Unemployment Insurance--A Legislative History: 1935-1985, Unemployment Insurance Service. 86-5
NTIS PB87-179834/AS.

Stephen A. Wandner, (editor) Measuring Structural Unemployment, Unemployment Insurance Service. NTIS PB87-209433/AS. 86-6

1987

Burt Barnow and Wayne Vroman, An Analysis of UI Trust Fund Adequacy, Unemployment Insurance Service. NTIS PB87-209342. 87-1

Esther Johnson, Short-Time Compensation: A Handbook Basic Source Material, Unemployment Insurance Service NTIS PB88-163589. 87-2

1988

Walter Corson, Stuart Kerachsky and Ellen Eliason Kisker, Work Search Among Unemployment Insurance Claimants: An Investigation of Some Effects of State Rules and Enforcement. Mathematica Policy Research. NTIS PB89-160022/AS. 88-1

UI Research Exchange. Information on unemployment insurance research. 1988 issue. Unemployment Insurance Service. NTIS PB89-160030/AS. 88-2

Walter Corson and Walter Nicholson, An Examination of Declining UI Claims During the 1980s. Mathematica Policy Research. NTIS PB89-160048/AS. 88-3

Phillip Richardson, Albert Irion, Arlen Rosenthal and Harold Kuptzin, Referral of Long-Term Unemployment Insurance (UI) Claimants to Reemployment Services. First Edition. Macro Systems and Mathematica Policy Research. NTIS PB89-153100/AS. 88-4

1989

Walter Corson, Walter Nicholson and Stuart Kerachsky, The Secretary's Seminars on Unemployment Insurance. Mathematica Policy Research. NTIS PB90-216649. 89-1

Phillip Richardson, Albert Irion, Arlen Rosenthal and Harold Kuptzin, Referral of Long-Term Unemployment Insurance (UI) Claimants to Reemployment Services. Second Edition. Systems and Mathematica Policy Research. NTIS PB89-153100/AS. 89-2

Walter Corson, Shari Dunstan, Paul Decker, and Anne Gordon, New Jersey Unemployment Insurance Reemployment Demonstration Project. Mathematic Policy Research. NTIS PB90-216714. 89-3

UI Research Exchange. Information on unemployment insurance research. 1989 issue. Unemployment Insurance Service. NTIS PB90-114125/AS. 89-4

John L. Czajka, Sharon L. Long, and Walter Nicholson, An Evaluation of the Feasibility of a Substate Area Extended Benefit Program. Mathematic Policy Research. NTIS PB90-127531/AS. 89-5

Wayne Vroman, Experience Rating in Unemployment Insurance: Some Current Issues. The Urban Institute. NTIS PB90-216656. 89-6

Jack Bright, Leadership in Appellate Administration: Successful State Unemployment Insurance Appellate Operations. Unemployment Insurance Service. NTIS PB90-161183/AS. 89-7

1990

Geoffrey L. Hopwood, Kansas Nonmonetary Expert System Prototype. Evaluation Research Corporation NTIS PB90-232711. 90-1

Esther R. Johnson, Reemployment Services To Unemployed Workers Having Difficulty Becoming Reemployed. Unemployment Insurance Service. NTIS PB91-106849. 90-2

Walter Corson, and Mark Dynarski, A Study of Unemployment Insurance Recipients and Exhaustees: Findings from a National Survey. Mathematica Policy Research, Inc. 90-3
NTIS PB91-129247.

UI Research Exchange. Information on unemployment insurance research. 1990 issue. 90-4
Unemployment Insurance Service.
NTIS PB91-153171.

1991

Patricia Anderson, Walter Corson, and Paul Decker, The New Jersey Unemployment Insurance Reemployment Demonstration Project Follow-Up Report. 91-1
Mathematica Policy Research, Inc.
NTIS PB91-160838/AS.

Wayne Vroman, The Decline In Unemployment Insurance Claims Activity in the 1980s. The Urban Institute. 91-2
NTIS PB91-160994/AS.

NOTE: A public use data tape also is available from the Bureau of the Census. To obtain the tape contact Customer Services, Bureau of the Census, Washington, D.C. 20233 or telephone 301-763-4100; when requesting the public use tape cite: Current Population Survey, Unemployment Compensation Benefits: May, August and November 1989 and February 1990 (machine readable data file) conducted by the Bureau of the Census for the Employment and Training Administration, U.S. Department of Labor, Washington: Bureau of the Census (producer and distributor), 1990.

Bruce H. Dunson, S. Charles Maurice, and Gerald P. Dwyer, Jr., The Cyclical Effects of the Unemployment Insurance (UI) Program. Metrica, Inc. 91-3
NTIS PB91-197897.

Terry R. Johnson, and Daniel H. Klepinger, Evaluation of the Impacts of the Washington Alternative Work Search Experiment. Battelle Human Affairs Research Centers. 91-4
NTIS PB91-198127/AS.

1992

Walter Corson, Paul Decker, Shari Dunstan and Stuart Kerachsky, Pennsylvania Reemployment Bonus Demonstration Final Report. Unemployment Insurance Service. NTIS PB93-152684. 92-1

Stephen A. Wandner, (editor) Self Employment Programs for Unemployed Workers. Unemployment Insurance Service. NTIS PB92-191626/AS. 92-2

Employer Layoff and Recall Practices. Bureau of Labor Statistics. NTIS PB92-174903/AS. 92-3

UI Research Exchange. Information on Unemployment Insurance research. 1992 issue. Unemployment Insurance Service. NTIS PB93-117968. 92-4

Wayne D. Zajac and David E. Balducchi, (editors) Papers and Materials Presented at the Unemployment Insurance Expert System Colloquium, June 1991. Unemployment Insurance Service. NTIS PB93-202695. 92-5

Robert G. Spiegelman, Christopher J. O'Leary, and Kenneth J. Kline, The Washington Reemployment Bonus Experiment Final Report. Unemployment Insurance Service. NTIS PB93-159499. 92-6

Paul T. Decker and Christopher O'Leary, An Analysis of Pooled Evidence from the Pennsylvania and Washington Reemployment Bonus Demonstrations. Mathematica Policy Research, Inc. and W.E. Upjohn Institute for Employment Research. NTIS PB93-160703. 92-7

1993

Paul L. Burgess and Stuart A. Low, Unemployment Insurance and Employer Layoffs. Arizona State University, Department of Economics. NTIS PB93-205573. 93-1

John G. Robinson, New Forms of Activity for the Unemployed and Measures to Assist the Creation of Self-Employment: Experiences and Opportunities in Combatting Unemployment. 93-2
Unemployment Insurance Service.
NTIS PB94-145299.

1994

Jacob M. Benus, Michelle Wood, and Terry R. Johnson, First Impact Analysis of the Washington State Self-Employment and Enterprise Development (SEED) Demonstration. 94-1
Abt Associates.
NTIS PB94-162203.

Walter Corson and Anu Rangarajan, Extended Benefit Triggers. 94-2
Unemployment Insurance Service.
NTIS PB94-178290.

Jacob Benus, Michelle L. Wood, and Neelima Grover, Self-Employment as a Reemployment Option: Demonstration Results and National Legislation. 94-3
Abt Associates.
NTIS PB94-188679.

The Worker Profiling and Reemployment Services System: Legislation, Implementation Process and Research Findings. 94-4
Unemployment Insurance Service.
NTIS PB95-172730.

U.S. Department of Labor
Employment and Training Administration
Washington, D.C. 20210

Official Business
Penalty for Private Use, $300

A-130

Fourth Class Mail
Postage and Fees Paid
U.S. Department of Labor
Permit No. G-736

BEST COPY AVAILABLE